

THE COMPARATIVE EFFECTS OF COACHING AND PRACTICE ON THE RESULTS OF VERBAL INTELLIGENCE TESTS

BY STEPHEN WISEMAN AND JACK WRIGLEY

Department of Education, University of Manchester

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I. INTRODUCTION

It has long been known that practice and coaching affect the scores on verbal intelligence tests. Rodger⁽⁶⁾ obtained a mean rise of 8 points of I.Q. from the first to the sixth Moray House test with 76 children aged 11-12 years. Bowers & Woods⁽¹⁾, analysing results from 1734 freshmen, concluded that experience in taking tests improves scores, but that after three or four tests there was no apparent increase. Vernon⁽⁸⁾, testing some 300 training-college students with N.I.I.P. Group Test 33 and Stephenson's Perceptual Test, showed that a consistent improvement occurred after instruction in the tests, and notes that 'the results indicate alarming possibilities of derangement of the norms for superior-adult tests'. He usefully distinguishes between normal practice effect and 'test-sophistication'—a factor that may be more important with schoolchildren than with adults. McRae⁽²⁾ used seven different types of test (Dawson, Scottish Mental Survey, Moray House, National, Otis, Simplex and Terman) with 130 children aged 10-12 years over a period of 2 months. In addition to demonstrating that the tests were not uniformly standardized, he concluded that the practice effect from six tests was 'more than 6 points'. Some of his groups received coaching as well as practice, but 'coaching seems to have no more effect than practice unless, perhaps, where the alternative form is used immediately before the examination'. He suggests that 'the first, and probably also the second, test of a series... acts as a shock-absorber'; but we are not told the degree of test-sophistication of his subjects (i.e. whether they had previously had experience of intelligence tests). In Rodger's investigation, already mentioned, this latter point is not clear, and its importance is obvious in view of the fact that the practice effect appears to be much larger from the first test to the second. A most important survey of unpublished research by Watts and by Navathe has been given by Vernon⁽⁹⁾. He concludes that the practice effect from the first test to the second averages 4-5 I.Q. points, and that 'further, but progressively smaller, increases occur with additional practice tests. After 4 to 5 tests the gain usually reaches a maximum of about 10 points...Bright children tend to benefit more than dull'. The effect of coaching seems to be much larger than this: an average rise of 14 I.Q. points, ranging up to 18 points in 'brighter and entirely unsophisticated pupils'. The gains from coaching do not appear to rise after the first three or four hours, and 'there seems to be little difference between different teachers as coaches'.

II. PURPOSE OF THE EXPERIMENT

In view of the greatly increased practical importance of this problem since the Education Act of 1944, it was decided by the Research Committee of the School of Education, University of Manchester, that an investigation should be made with the co-operation of education authorities and schools within its area. The experiment was designed to reveal the amount of improvement in I.Q.'s (derived from standard intelligence tests) which could be obtained by (a) giving children simple practice in doing similar tests, without knowledge of results, and (b) coaching children in the items found in published books containing 'intelligence' questions. It should be noticed that other experiments on this problem have used 'coaching' to mean the teaching of the actual types of item used in the tests themselves. The present design was adopted deliberately in order to make the experiment more realistic in its results. A number of books are now published which purport to teach children how to do 'intelligence tests', and these appear to be bought in quantity by teachers and parents. The sales of all reputable intelligence tests which are used for 11 plus selection are restricted, and they cannot be bought by individual teachers or parents. It would, of course, be foolhardy to suggest that no teacher possesses copies of, say, Moray House Intelligence tests, but it is almost certain that coaching is more often based on the books which may be bought over the counter than on the tests which are actually used for selection. Care must be taken, however, not to equate 'coaching' as defined in this experiment with the 'coaching' used in other experiments. One would obviously expect greater gains from 'test coaching' than from 'book coaching'.

The experiment was planned to deal with children as near as possible to the age of transfer to secondary schools, since this is the age to which practice and coaching is almost entirely confined. This raises severe problems. Previous research shows that the effect of practice is greatest from the first test to the second, and gradually falls off as further tests are done. Therefore it is desirable to use children who have not yet been 'exposed' to intelligence tests. This rules out the use of children who have already taken the selection examination. On the other hand, it is necessary to guard against the danger that the coaching of children for experimental purposes might affect their performance in the official selection test, and give them an unfair advantage over their fellow-pupils. Another factor which had to be taken into account was the amount of disorganization which schools might suffer as a result of the experiment. The splitting of classes and the regrouping of children is not only to be deprecated because of the upset caused to other 'non-experimental' children and to the school staff, but also because it introduces more artificiality into the experiment and might seriously affect the motivation of the children. Since the aim of the experiment was to measure the effect of any practice or coaching which *might* be performed in a school, it follows that the experimental practice and coaching should be carried out in the same way as it would be in the ordinary school programme—that is, using complete classes as normally constituted.

Taking all these points into consideration, it was decided to use children in the primary schools who were in the age-group immediately below the class which was to take the special place examination in 1951, and to do the experiment as early as possible in 1951. This would allow well over a year to elapse between the experiment and the official selection examination, since the experimental classes would not take this examination

until 1952. Only one exception was made to the 'complete class' rule, and that was the exclusion of any child who would be taking the 1951 examination as an 'under-age' candidate. This was obviously essential. One disadvantage of this experimental plan was that the normal intelligence tests designed for 11 plus selection would be too difficult for the younger and the duller children. This handicap was accepted, since the emphasis in the investigation was on the upper half of the ability-range—i.e. on those children who were at least 'possibles' for grammar-school selection. The effect of coaching and practice on children below 85 I.Q. is of academic interest to the teacher, even though it is of real interest to the psychologist.

III. DESIGN OF THE EXPERIMENT

Fifteen teachers attending evening classes at the Education Centre volunteered to take part in the experiment. These teachers came from thirteen schools within four local education authorities.

A number of books on 'intelligence' were bought from local bookshops and reviewed. Among the best of these was the *Kingsway Intelligence Tests* (published by Evans Brothers), and it was decided to use this. There were included in this book, however, a number of item types which were obviously unlikely to contribute much to any coaching programme, and it was decided to omit six types from the coaching lessons (question types 5, 11, 12, 14, 15 and 17 in the sample test at the beginning of the book). The experiment, then, used the *best* book out of a number, and then omitted the *worst* elements within this. Therefore any results obtained are likely to be greater than those obtainable from less selective use of this kind of published material.

The classes were divided into three groups, a practice group, a coaching group and a control group. All classes were given an initial intelligence test, and a final (different) intelligence test at the end of the period of the experiment. It was decided that this period should be one of 6 weeks, and that 1 hour per week should be spent on practice or coaching. It seemed likely that six practice tests, plus initial and final tests, would come near enough to exhausting the practice increase for the purposes of the experiment, and a longer experimental period would give very sharply diminishing returns for the extra effort demanded of the schools. The practice group, therefore, had a different intelligence test administered each week for 6 weeks; the coaching group had 1 hour of coaching per week for 6 weeks, and the control group carried on normally with its ordinary time-table for 6 weeks. The practice group were given *practice only*; i.e. the children had no knowledge of their results, and no revision of the tests was done. All the tests used were Moray House Tests: Moray House Test 43 for the initial test, Moray House Test 44 for the final test, and Moray House Tests 37, 38, 39, 40, 41 and 42 for the practice tests.

It is known that the effect of practice and coaching—on all kinds of scholastic material—is correlated with the level of intelligence of the children concerned. It was therefore necessary to ensure (as far as possible) that the three groups did not differ markedly in the distribution of intelligence. The use of complete classes made 'matched groups' impossible, but it was hoped that an adequate equating of ability between the groups could be achieved by the judicious allocation of classes to groups. This was done on the basis of the results on the initial intelligence test.

IV. THE EXPERIMENTAL TIME-TABLE

The initial intelligence test was given in all classes during the third week in January. A number of factors intervened to make it impossible to start the experimental period until the week beginning 19 February. This meant that Easter holidays would prevent an unbroken stretch of 6 weeks to be spent on the experiment. A break of 3 weeks was therefore made between the fourth and fifth experimental weeks. The final time-table was as follows:

Initial test	22-26 January
Practice or coaching	1 19-23 February
	2 26 February-2 March
	3 5-9 March
	4 12-16 March
	5 9-13 April
	6 16-20 April
Final test	23-27 April

V. ESTIMATES OF COACHING EFFICIENCY

It was thought desirable to arrange for visits to be made to the classes, particularly those in the coaching group, in order to see if any major differences existed in the methods of teaching adopted. The teachers taking part had all met together on several occasions, and the instructions given—both verbal and written—tried to ensure some degree of uniformity in the carrying out of the experiment, but quite obviously it was impossible (and undesirable) to attempt to stereotype the instruction given in the coaching periods. Each teacher rightly uses his or her own methods in order to achieve a common aim. One of us (J.W.) therefore visited all coaching classes during the period, and a (subjective) estimate of the efficiency of the instruction was made. This was done in case large inter-class differences were shown in the results, which might have been due to different approaches. It was noted that in practically all classes the teachers and pupils were interested and even enthusiastic. ‘Puzzles’ appeared to be one of the most popular lessons of the week. The brighter children appeared, on the whole, to enjoy the lessons more than the dull ones. It was evident, too, how difficult it is to ‘teach’ some kinds of intelligence test items, and how different teachers use widely different approaches in coaching, say, number series.

VI. EXPERIMENTAL RESULTS

(i) *Intelligence levels in the three groups*

After the initial test had been given, classes were allocated to the three groups in such a way as to give approximately equal distributions of i.q. Complete freedom in this allocation was not possible, since two classes participated in the experiment on the understanding that they should be used only as control classes. The mean i.q.’s for the classes in the three groups are given in Table 1, together with mean ages and i.q. ranges.

It will be seen that the practice group is 5 points of i.q. below the other two groups. This is unfortunate, but not serious: as will be seen later, it was found possible to calculate the practice effect at different levels of initial i.q.

Quite a number of children fell below the age of 9.6. This made it impossible to convert their scores to I.Q.'s, since the conversion table does not extend below that age. In any case, the results of children below this age on a test designed for 10-11 years are unlikely to yield information on which any degree of confidence can be based. Other children who

Table 1. *Initial intelligence level in the three groups*

Control					Practice					Coaching				
Class	No.	Mean age	Mean I.Q.	I.Q. range	Class	No.	Mean age	Mean I.Q.	I.Q. range	Class	No.	Mean age	Mean I.Q.	I.Q. range
1	38	10.0	94.6	77-118	6a, b	54	9.8	95.1	78-114	9	37	9.7	95.5	81-104
2	37	9.11	117.0	105-130	7a	30	9.8	107.2	94-129	10	13	9.11	89.2	70-106
3a, b	45	9.9	100.0	70-121	7b	37	9.10	88.9	74-109	11a	41	9.11	110.7	98-131
4	27	9.9	89.6	80-114	8a, b	71	9.10	100.1	83-130	11b	36	9.7	94.6	70-111
5	33	9.11	100.8	80-121	—	—	—	—	—	12	32	9.10	102.2	83-121
—	—	—	—	—	—	—	—	—	—	13	27	9.7	99.6	85-118
Total	170	9.10	100.0	70-130	—	192	9.9	95.1	74-130	—	186	9.8	100.0	70-131

Total for all three groups (eighteen classes from thirteen schools):

No.	Mean age	Mean I.Q.	I.Q. range
548	9.9	98.3	70-131

were present for the initial test were absent for subsequent tests or coaching periods. As a result of these two sources of wastage the total numbers available for analysis at the conclusion of the investigation were:

	Control	Practice	Coaching	Total
No.	140	112	129	381
Mean I.Q.	101.9	99.6	102.2	101.0

(ii) *Increases in mean score*

The first and most obvious analysis is to take the mean I.Q. of each group on the initial test and compare it with the mean I.Q. on the final test. This will give the over-all improvement due to practice or coaching. These results are:

	Initial test	Final test	Rise in I.Q.
Control group	101.92	106.60	4.68
Practice group	99.62	110.71	11.09
Coaching group	102.16	108.53	6.37

The rise in the control group is $4\frac{1}{2}$ points—the practice effect due to the first test. (This may be compared with the practice effect from the initial test to the first practice test for the practice group, which was $4\frac{1}{4}$ points of I.Q.) The coaching group shows a rise of just over 6 points, and the practice group increases by 11 points. The importance of the kind of coaching material used is emphasized by this result, since it seems likely that the greater gains reported from other coaching experiments are due to this factor. This experiment, however, seems to indicate fairly clearly that the effectiveness of coaching from published books on 'intelligence tests' is over-valued by many teachers. Notice that the total effect of coaching may be regarded as $6.37 - 4.68 = 1.69$ points of I.Q. This hardly seems an adequate gain from six lessons' work.

(iii) *The practice effect*

The average I.Q.'s for the successive practice tests given to the practice group are shown below.

Initial test	Practice tests						Final test
	2	3	4	5	6	7	
99.62	104.07	105.88	105.73	110.84	109.05	110.57	110.71

A curve was fitted to the data above. It appeared likely that a good fit might be obtained from a hyperbola having the general formula

$$y = \frac{x}{a + bx} + c,$$

or a logarithmic growth curve of the type $y = a + bx + c \log x$. Both types of curve were tried, and the following equations obtained:

$$y = \frac{x-1}{0.0978 + 0.0644x} + 99.62, \quad (a)$$

and

$$y = 99.853 - 0.1723x + 13.8638 \log x. \quad (b)$$

The sum of the squared deviations from curve (a) was 9.4301, and from curve (b) 9.8024, showing that, while there was little to choose between them, the hyperbola gave a slightly better fit. From this we may estimate that the gains from successive tests are:

	Points of I.Q.		Points of I.Q.
1st to 2nd	4.4	5th to 6th	0.8
2nd to 3rd	2.5	6th to 7th	0.6
3rd to 4th	1.6	7th to 8th	0.5
4th to 5th	1.1		

The curve is flattening out by the eighth test, and we may judge the maximum practice effect to be 11 points of I.Q. Notice that the first four tests contribute 8.44 points to this total, and the first five 9.53 points. This suggests that three or four tests given before any selection test will succeed in absorbing the major part of the practice effect. This may only be strictly true, of course, for tests given weekly immediately before the examination. The experiment does not show what degree of permanence is attached to these gains. It is very doubtful whether tests given at 7 and 9 years for record card purposes will have any residual effects at the age of 10 or 11.

(iv) *The effect at different intelligence levels*

It is obvious that, for selection purposes, we are more interested in the effects of practice and coaching at the 110–120 I.Q. level than at the lower levels, since this is the group which normally forms the ‘borderline’ in selection. In most experiments on the effects of learning it has been shown that the gains achieved are related to the level of ability of the children, and that, generally speaking, the brighter children gain more than the duller. The gains from practice and coaching which have already been reported are, of course, in terms of averages derived from complete groups containing a wide range of ability. It may be that the gains in the upper levels are much greater than these.

It is not easy to calculate the gains at different intelligence levels. The obvious approach—dividing the children into levels on the basis of the initial test (say, I.Q. 85–99, 100–114, 115–129, etc.) and then finding the average gain in each level—gives a most misleading picture. This is due to the fact that initial and final tests are correlated, and the regression effect depresses the gains made by the brighter pupils and raises the gains of the duller groups. The same effect is present if we work backwards, so to speak, by grouping on I.Q.’s on the final test. (An excellent account of this regression effect in experiments on learning has been given by Philpott(4).)

The gains at different I.Q. levels were calculated by three methods:

(i) The 'level' of ability was defined by taking the sum of the quotients for the initial and final tests. Thus, if x is the score on the initial test and y the score on the final test, $(x+y)$ was found for each child. The $x+y$ scores were now placed in rank order, and divided into levels (e.g. 180–199, 200–219), and the average gain $(y-x)$ for each level calculated. By plotting these points graphically it can easily be seen whether there is any rise or fall in $(y-x)$ with increase in $(y+x)$, and by fitting a straight line, or curve, to the points, the gain at any particular ability level may be estimated. It is necessary, of course, to re-translate the $x+y$ score into an I.Q. score on the first test in order to make the results meaningful. One way of doing this would be to find the average value of x (I.Q. on initial test) for each class-interval of $x+y$. A preferable method is to find equivalent x values from the regression of x on $(x+y)$. This was done.

(ii) The second method is one devised by Peel (3). This is a complicated method, which takes into account the correlation between x and y , and calculates the practice effect by the solving of quadratics by successive iteration.

(iii) Peel (3) also describes a shorter method devised by Gourlay, in which the practice effect is calculated by a comparison of percentile levels on the first test with the same percentile levels on the second. Unlike the other two methods, this compares distributions rather than individual scores. It was used as a check on methods (i) and (ii).

When the graphs were plotted for the control group, the practice group and the coaching group, it was evident that methods (i) and (iii) gave results which were in close agreement, while method (ii) showed quite large divergencies. It was therefore decided to continue the analysis using method (i). The data for the three methods are shown in Table 2.

For each of the experimental groups, the linearity of regression was tested, using analysis of variance. The results are shown in Table 3, from which it will be seen that the results for the practice and coaching groups do not depart significantly from linearity, but that in the control group there is strong evidence of curvilinear regression ($F=7.384$; 1% $F=4.75$). A second-degree polynomial was fitted to the control data: it was found that a third-degree curve did not produce a significant reduction in the sum of squares (using the method given by Snedecor (7), p. 391). Straight lines were fitted to the data of the coaching and practice groups. The resulting equations are as follows:

$$\text{Control: gain} = 0.00601 x^2 - 1.216 x + 65.046.$$

$$\text{Practice: gain} = 0.357 x - 24.558.$$

$$\text{Coaching: gain} = 14.885 - 0.0763 x.$$

These curves are drawn in Fig. 1.

The relative agreement of the three methods of calculation already described may be shown by obtaining the sum of the squares of the deviations of the plotted points from these lines. Since the methods yielded different numbers of points in each group, the average per point was calculated. The results are shown in Table 4. Naturally the deviations are least for method (i) since the curves were fitted to the points obtained by this method, but the results bear out the close agreement of the methods (i) and (iii) which has already been noted.

The graph shows that the gains from practice increase very rapidly as the I.Q. level rises, and that at I.Q. 130 the gain is over 20 points. The reverse trend is shown for

coaching, where the graph has a slight negative slope. At the 120 I.Q. level the coaching graph crosses the control graph, which suggests that at this level the effect of coaching is

Table 2. *Gains at different I.Q. levels, calculated by three methods*

Control group										
Method (i)										
Initial I.Q.	82.13	92.26	102.39	112.53	122.66					
Gain	5.40	4.98	2.93	4.33	6.50					
Method (ii)										
Initial I.Q.	82	87	92	97	102	107	112	117	122	127
Gain	1.51	4.93	3.73	4.63	3.85	1.52	8.63	6.02	4.75	6.08
Method (iii)										
Initial I.Q.	82	87	92	97	102	107	112	117	122	127
Gain	2.99	4.40	4.64	5.66	3.67	3.44	4.67	5.68	5.82	8.63
Practice group										
Method (i)										
Initial I.Q.	82.22	90.64	99.06	107.48	115.90					
Gain	5.39	5.73	12.39	14.42	16.07					
Method (ii)										
Initial I.Q.	87	92	97	102	107	112	117			
Gain	6.14	8.60	9.06	11.62	13.15	24.35	27.90			
Method (iii)										
Initial I.Q.	79.5	84.5	89.5	94.5	99.5	104.5	109.5	114.5	119.5	
Gain	5.00	6.15	5.56	8.89	13.00	13.12	14.00	13.85	18.00	
Coaching group										
Method (i)										
Initial I.Q.	81.05	91.37	101.69	112.02	122.34					
Gain	10.13	7.28	5.41	5.94	6.86					
Method (ii)										
Initial I.Q.	87	92	97	102	107	112	117	122		
Gain	6.43	10.93	7.26	6.74	4.31	9.83	1.87	11.31		
Method (iii)										
Initial I.Q.	84.5	89.5	94.5	99.5	104.5	109.5	114.5	119.5	124.5	
Gain	7.86	6.76	6.15	6.67	6.82	7.05	6.11	8.33	8.33	

Table 3. *Linearity of regression*

Source of variation	Sum of squares	Degrees of freedom	Mean square	F
Control group				
Linear regression	6.605	1		
Additional variance	70.961	2	35.481	7.384**
Total variance between groups	77.566	3		
Within groups	639.120	133	4.805	
Total	716.686	139		
Practice group				
Linear regression	158.426	1		
Additional variance	24.325	3	8.108	2.119 n.s.
Total variance between groups	182.751	4		
Within groups	394.169	103	3.827	
Total	576.920	111		
Coaching group				
Linear regression	26.173	1		
Additional variance	16.694	3	5.565	1.09 n.s.
Total variance between groups	42.867	4		
Within groups	611.750	120	5.098	
Total	654.617	128		

insignificant. As the level of initial I.Q. decreases, however, the gains from coaching increase, and below about 90 I.Q. it appears as if coaching is more effective than practice. Table 5 shows the gains of the three groups at six I.Q. levels, as read from the graph.

From these results we may estimate the relative effectiveness of practice or coaching by subtracting the 'control gain': i.e. by measuring increases from the control line. In round figures this shows that at 90 I.Q., the practice net gain is practically identical with the coaching net gain at 3 to $3\frac{1}{2}$ points; at 100 I.Q. the practice gain is $7\frac{1}{2}$ points and the coaching gain 4 points; at 120 I.Q. the practice gain is $12\frac{1}{2}$ points and the coaching gain nil.

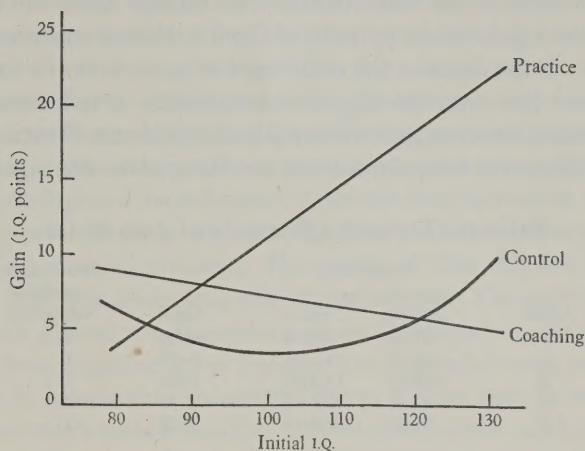


Fig. 1.

Table 4. *Sums of squared deviations*

	Method		
	(i)	(ii)	(iii)
Control	0.25	5.11	1.48
Practice	1.33	27.64	1.65
Coaching	1.51	10.93	2.02

Table 5. *Gains at different I.Q. levels from best-fitting lines*

I.Q.	Control	Practice	Coaching
80	6.2	3.9	8.8
90	4.3	7.5	8.0
100	3.5	11.1	7.2
110	4.1	14.7	6.5
120	5.8	18.3	5.7
130	8.5	21.9	5.0

It is, of course, difficult to generalize from these results. It may be that the teachers of the coaching classes tended to concentrate their efforts on the weaker and duller pupils. We know that individual teachers vary in their techniques when teaching groups with a wide range of ability: some teach 'to the average', others try to extend the abler pupils, others again devote more time to the slow learners. Certainly the instructions to the teachers gave no suggestions on this matter, and perhaps we may conclude that the teachers concerned did their best to coach all grades of ability within their classes. If the sample had been larger, it would have been possible to analyse results for each teacher. With the present experiment, however, the numbers in levels within classes is too small to permit this further analysis.

Our main interest is at the 'borderline' level for 11 plus selection. Taking 115 I.Q. as

being a fairly typical 'pass mark' we find from the graph that the net gain for practice is 12 points as against $1\frac{1}{2}$ points for coaching. It is clear that practice is likely to be much more effective than coaching in lifting a doubtful candidate above the pass mark.

(v) *The effect of teaching efficiency*

It will be remembered that estimates of teaching efficiency were made by one of the writers during the course of the experiment. The ratings made are shown in Table 6, together with the mean gain made by each of the five classes concerned. The rank order of teaching efficiency is the same as the rank order of gain, with the exception of class *R*. With only five classes, however, the suggested relationship is not statistically significant. An analysis of variance between and within schools yields an *F* ratio of 1.41 (5% point 2.29), so that the differences between schools are themselves not significant.

Table 6. *Teaching efficiency and gain in I.Q.*

Class	Mean I.Q.		Gain	Estimate of teaching efficiency
	Initial	Final		
<i>P</i>	91.79	98.58	6.79	B +
<i>Q</i>	105.91	111.88	5.97	B
<i>R</i>	102.52	110.35	7.83	C +
<i>S</i>	99.21	104.68	5.47	C -
<i>T</i>	97.22	102.44	5.22	D -

VII. DISCUSSION

Many of the data from this inquiry confirm previous findings. For example, it is shown, both from the control group and the practice group, that the practice effect of a single test is $4\frac{1}{2}$ points of I.Q.: Vernon (9), reviewing other recent research, suggests 4 to 5 points. He finds, too, that continued practice tests produce a maximum gain of about 10 points: here the mean increase was 11 points. These close agreements suggest that some degree of confidence may be placed on the experimental and statistical methods, and on the sample of children used. It is, therefore, all the more interesting to observe the difference between the results obtained from coaching on published books and those from coaching on parallel forms of the test. The small effect of the former—so small that for children above average intelligence the gains are of no practical significance—contrasts very strikingly with the 14 to 18 points rise in I.Q. reported by Vernon for parallel-test coaching. It seems clear that the selector need not fear an increase in the number of misfits in the grammar school as a result of coaching of this type, whether carried out by parents, teachers or professional coaches. The use of two or three practice tests before the selection examination would quite clearly swamp this coaching effect at all levels of I.Q.

It has been suggested (9) that in order to obviate injustices caused by parallel-test coaching 'official' coaching ought to be introduced into the schools. There is much to be said for trying other methods of selection before considering such a drastic step. There are two main arguments against official coaching. The first is that it might well accentuate differences between schools—and from the selector's point of view one of the virtues of the intelligence test is to reduce such differences. Although the present experiment was unable to show a significant connexion between teaching efficiency and gain in I.Q., the results are suggestive and call for a further test of this hypothesis. The second argument concerns the essential difference between the intelligence tests and tests of attainment

The psychologist has been stressing this difference for many years, and has been trying to impress on teachers that whereas tests of arithmetic and English are to a large extent measures of learning and teaching, the intelligence test is not. This is always a difficult point for teachers to accept, since their whole professional life leads them to expect measurable gains from teaching—with *any* material. The psychologist and administrator have always resisted their natural desires to teach 'intelligence', not only because of the possible upset to norms and standards, but also—and mainly—because it is largely a waste of time (Vernon suggests that 3 or 4 hr. is likely to absorb the whole of the coaching effect). If an authority institutes coaching of 3 or 4 hr. duration only, it seems extremely unlikely that teachers will stop at this point. The official approval of coaching will fortify the teacher's natural professional belief in his powers to produce improvement, and since continued teaching in arithmetic produces continued gains over periods of months and years, why should 'intelligence' be different? Once the door is opened, control of coaching time is unlikely to prove effective, and the 'intelligence lesson' every week (or oftener) will become more and more in evidence. The results of this will not be confined to the primary school, nor will they affect only the 11 plus selector. The use of group intelligence tests in the secondary school, for diagnosis and for classification, may be vitiated.

Since large gains from coaching come from the use of parallel tests, one safeguard which may be employed is to ensure that stereotyped tests are not used in selection. A change in item type from one year's tests to another is quite practicable—and, of course, adequate security measures so that selection tests are not available to unauthorized persons. Another important function of the local education authority would be the demonstration to teachers from time to time of the essential difference between intelligence tests and attainment tests. One local authority⁽⁵⁾ has shown, with long-term coaching, that although the mean i.q. only rose 3 points after 20 weeks, the arithmetic quotient showed a rise of 20 points (with the paired control classes in the same schools rising 18 points!).

VII. SUMMARY OF CONCLUSIONS

1. The mean gain in i.q. in the control group (3-month period) was $4\frac{1}{2}$ points.
2. The mean gain for the coaching group was $6\frac{1}{2}$ points.
3. The mean gain for the practice group, after eight tests, was 11 points.
4. The first five practice tests produced a gain of $9\frac{1}{2}$ points.
5. The effect of practice was highly dependent upon initial level of ability; the higher the i.q. the greater the gain. At i.q. 120 the gain was $18\frac{1}{2}$ points.
6. This effect was reversed in the coaching group: the gain was nearly 9 points at i.q. 80 and $5\frac{1}{2}$ points at i.q. 120.
7. There was a positive correlation between teaching efficiency (as rated by an observer) and the gains produced from coaching. This was not statistically significant.

ACKNOWLEDGEMENTS

We are grateful to the authorities concerned for permission to carry out the experiment, and to the following teachers who took part: M. Allen, B. R. Bedford, F. Gleeson, J. Greenough, C. Hall, G. Hodgkinson, A. Holden, R. Irving, A. Pilling, H. Pilling, F. Rawlinson, B. Reynolds, J. S. Seaton, E. L. Thomas and M. E. Young. We are also grateful to Moray House and to the University of London Press for their whole-hearted co-operation.

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(Manuscript received 5 July 1952)

SOCIAL CLASS ATTITUDES AND LEVELS OF OCCUPATIONAL ASPIRATION IN SECONDARY MODERN SCHOOL LEAVERS¹

BY GUSTAV JAHODA
University College of the Gold Coast

I. *Aims and methods* (pp. 95-98): (i) *Introduction* (p. 95); (ii) *Social class grading* (pp. 95-97); (iii) *Social class scale* (pp. 97-98). II. *Social status and class identification* (pp. 98-102): (i) *Introducing the class scale* (pp. 98-99); (ii) *Membership groups and reference groups* (pp. 99-100); (iii) *Correction of scale values and results* (p. 101); (iv) *Summary of class attitudes* (pp. 101-102). III. *Levels of social and occupational aspiration* (pp. 103-106): (i) *Introduction* (pp. 103); (ii) *'Autistic Aim' on the class scale* (pp. 103-105); (iii) *Realistic expectations on the class scale and verbal statements of aim* (pp. 105-106). IV. *Summary* (pp. 106-107).

I. AIMS AND METHODS

(i) *Introduction*

The aim of the present investigation was to study adolescents' ideas and feelings about their social status. An attempt was made to find out what they conceived it to be in the present, as well as their wishes and anticipations for the future.

The inquiry was carried out during 1949-50 in a north-west Lancashire borough of about 62,000 inhabitants, an important commercial and industrial centre specializing largely in heavy engineering and chemical products. The sample consisted of 56 boys and 77 girls, representing all midsummer 1950 leavers from the four main secondary modern schools.

Methods used, which were tested in a pilot survey, consisted of a series of specially devised techniques supplementing intensive individual interviews.² Contact was maintained with the young people over a period of 6 months, so that good *rapproch* could be established.

(ii) *Social class grading*

For the purpose of investigating class attitudes it was necessary to grade the young people according to their social status, which was taken to be the same as that of the head of the household in which they lived. Some measure of 'accorded status' or, in other words, of status as assessed by external criteria, was therefore needed.

Although the whole topic is a moot one³ there is a fair amount of agreement that occupation is the best *single* index of socio-economic status;⁴ it has also the great advantage of being rather easily and reliably ascertainable.

From among the several existing scales the Hall-Jones one (3) was selected, because it is at least partly based upon empirical research. It was adopted, not because it is above

¹ This report is an extract from a wider study concerned with the job attitudes and job choices of secondary modern school leavers, which was submitted for the degree of Ph.D. in the University of London.

² The techniques included a job attitude test labelled 'job-quiz', a sentence completion test and a card-ranking device. Raven's 1938 Matrices were also administered. For a more adequate discussion of methods reference may be made to the thesis in the University of London Library.

³ Cf. Centers (2), ch. II.

⁴ Cf. Hatt (4).

criticism, but because it appeared to be the best available. Its standard classification is as follows:

Type of occupation	H-J ¹
Professional and high administrative	1
Managerial and executive	2
Inspectional, supervisory and other non-manual, higher grade	3
Inspectional, supervisory and other non-manual, lower grade	4
Skilled manual and routine grades of non-manual	5
Semi-skilled manual	6
Unskilled manual	7

N.B. H-J 5 is a very comprehensive grade, which includes foremen, routine clerks or typists and the smaller types of business.

Each subject was classified in one of the above categories and Table 1 gives the distribution of what will be called 'objective class gradings'.

Table 1. *Objective class gradings of the sample*
(Number of individuals belonging to each grade.)

Sex	H-J grades							Total
	1	2	3	4	5	6	7	
Boys	1	2	1	3	25	14	10	56
Girls	—	1	4	6	37	24	5	77

Table 2. *Grouped H-J grades (Percentages)*

Sex	H-J groups				Total
	1, 2, 3, 4	5	6, 7		
Boys	(13)	(44)	(43)		(100)
Girls	(14)	(48)	(38)		(100)

Table 3. *Percentage of young people in the three H-J groups residing in the poorer areas*

Percentage residing in the poorer areas	H-J groups		
	1, 2, 3, 4	5	6, 7
	(22)	(63)	(68)

As the sample is a fairly homogeneous group the four highest and two lowest grades have usually been combined (Table 2).

In view of the fundamental importance of this classification, three checks, capable of showing up gross errors, were used:

- (a) Areas of residence.
- (b) Occupational status and I.Q.
- (c) Comparison with teachers' ratings.

(a) *Areas of residence*

With the assistance of the borough surveyor the town was divided into wealthy or average, and poor sub-areas, in accordance with housing-age and density. The proportion of boys and girls within the different H-J groups residing in the poorer areas was then plotted (Table 3).

Owing to the comparatively small numbers in the upper groups the differences are not statistically significant, but the trend is in the expected direction. A factor tending to reduce the difference between H-J 5 and H-J 6 and 7 is the existence of post-war housing estates. Through the working of the points system, people in H-J 6 and 7 with large

¹ H-J will be used throughout as an abbreviation for 'Hall-Jones Grade'.

families have a good chance of moving into them, out of the poorer overcrowded areas.

b) Occupational status and intelligence

The relation between children's I.Q. and the occupational status of their parents has often been investigated, with very similar results. If the present classification has been adequate, a correlation coefficient of the same order should be obtained. As it happened the product-moment correlation between H-J grades and the progressive matrices scores turned out to be exactly the same as the figure reported by Burt⁽¹⁾, namely +0.32 (P less than 0.001).

(c) Comparison with teachers' ratings

Teachers were asked to rate each boy and girl on a simple class scale. This was constructed in such a way that it could be directly compared with the H-J scale (at the same time occupational criteria were rigidly excluded, so as to ensure a genuine validation):

H-J grade	Corresponding rating in teachers' scale
1, 2, 3, 4	Lower middle class and above
5	Good working class
6	Average working class
7	Poor working class

The mean product-moment correlation between teachers' and the investigator's ratings was +0.55 (P less than 0.001). This may not appear very high, but two points should be taken into consideration. The word 'average', unfortunately used in the teachers' scale, brought about a convergence on to this category which accounts for a large part of the discrepancy. Furthermore, the scale required fine discrimination within a narrow range.

Since all three methods of validation gave positive results it was concluded that the grading was sufficiently accurate for further analysis.

(iii) Social class scale

This was one of the special techniques, developed in order to obtain an approximate measure of subjective class feeling. In its original form it consisted of a piece of card-board on which the seven H-J grades were printed in bold numbers. In small lettering, beside each, were examples of jobs falling into the various grades. The boys and girls were asked to indicate where they thought their families and friends belonged. This was unsuccessful, because the young people tended to start searching for specific jobs among the examples, although they had been instructed that these stood only for general types. Often they would not give any definite response, unless they found the job they were looking for. In any case, responses were far too closely tied to particular occupations, without sufficient consideration of their social standing, and no amount of detailed explanation could overcome this difficulty.

After repeated trials the scale was completely changed. All conventional examples of occupation were cut out, but top and bottom anchoring points were added. At first the subjects had to mark their position by a pencil line: then it was found that they sometimes wished to change their mind, so the whole scale was mounted on to a wooden board with a movable pointer fitted along the right-hand side (Fig. 1).

It will be noted that the points on the class scale are so arranged that they correspond to the H-J grades excepting the top and bottom anchoring points: the actual numerical relation is inverse:

Points on the class scale	(1)	2	3	4	5	6	7	8	(9)
Corresponding H-J grades	—	7	6	5	4	3	2	1	—

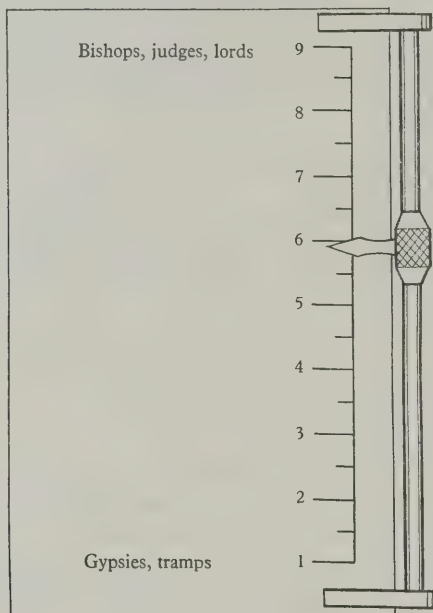


Fig. 1.

The subjects' judgements in 'raw' scale values within the range from 2 to 8 can therefore be transformed into H-J equivalents by subtracting them from 9.

Before the scale was adopted its test-retest reliability after an interval of 3 weeks was ascertained with twenty-four subjects. On both occasions each subject was required to make judgements on three different criteria. A mean reliability coefficient of +0.84 was obtained.

II. SOCIAL STATUS AND CLASS IDENTIFICATION

(i) *Introducing the class scale*

The part of the interview that dealt with social class was introduced by means of one of the items in the card-ranking device,¹ administered to each boy and girl. 'GOOD SOCIAL POSITION' was the card deliberately included for preparing the ground. It nearly always needed an explanation, given roughly as follows:

Most people say about some jobs (such as being the boss of a big factory, or the matron of a hospital) that if you have a job like that you are in a very good social position.

There are other jobs that are necessary and have to be done (such as dustman² and road-sweeper²) but people say you are *not* in a good position if you have a job like that.

¹ A modified form of one used by Wilkins⁽⁹⁾.

² Examples were varied occasionally so as to avoid any mention of occupations closely resembling those of the young people's parents or relatives.

Most boys and girls grasped this quite easily, many remarking spontaneously 'Yes, some jobs are high and others low'.

A mental set on the concept of 'social position' was thus created, which paved the way for the subsequent presentation of the class scale. The greatest care was taken to ensure that the principle of the scale was clearly understood: with most of the subjects this was not very difficult, but seven of the least intelligent boys and girls, who were unable to grasp it, had to be left out.

The general procedure of introducing the scale ran along the following lines:

Experimenter: You see this board I have here with numbers on it—it's like a ladder with a moving pointer.

Now, I move the pointer up to where it says bishops, judges, lords and people of that sort. What kind of position would you say that people like that have?

Subject: They're high-ups—they've got good positions.

Experimenter: Now I move it down to where it says gypsies and tramps: what kind of position would you say people like that have?

Subject: They're low: they haven't got any position.

At this point some subjects with a strong feeling for social equality would say 'They're just as good as lords—you have high people who are no good'. This was then discussed to put over the idea that the usual evaluation of a group by the majority of people was meant, and not a judgement passed on specific individuals. This was nearly always accepted readily.

Once everything was clarified the experimenter would go on:

Most of us are neither lords nor tramps, but somewhere in between. What I would like you to do now is to push the pointer to where you think *most of the people you know well would be*: say your friends, their parents, your own relations and so on. There is no right or wrong in this, just push the pointer where you feel they belong.

At this stage there was normally a pause for reflexion and occasionally slight anxiety was manifested, more often by girls than by boys. Once begun, the movement of the pointer was mostly slow and deliberate: there were usually a few changes before a definite position was decided upon.

It should be noted that the word 'class' was never mentioned. Moreover, the question was not put with reference to the subjects themselves, so that less ego-involvement would, it was hoped, result in a more detached scale-choice.

(ii) *Membership groups and reference groups*

The results of these 'self-choices' tabulated by sex and class, are now shown. In this and all subsequent tables (unless otherwise stated) raw scale values are transformed into H-J equivalents, so as to facilitate comparison.

'Common sense' might have suggested that the breakdown would show a consistent relation between objective class ratings and scale-choices. In fact, however, analysis of

Table 4. *Mean self-choices*

Objectively rated class groups	Boys			Girls		
	No.	Mean self- choices	S.D.	No.	Mean self- choices	S.D.
1-4	7	4.36	0.83	11	2.91	1.45
5	25	4.18	0.72	33	3.68	1.13
6, 7	22	4.45	1.04	28	3.48	1.26
1-7	54	4.32	0.90	72	3.49	1.26

variance brought out no significant class differences, although the sex difference proved significant at the 1% level. One is thus led to the conclusion that the scale-choices, if they mean anything, will provide a clue to subjective class identification. In other words they will indicate the young people's reference groups, rather than their membership groups whenever the two are not identical.

This can be tested by using a further set of data. After each subject had moved the pointer to its position on the scale, he was asked what had made him stop just there. The responses were grouped according to the broad class identification they implied.

A. Describes himself as above the middle, higher than the average and/or claims relations and/or friends 'high-up in life'.

B. Describes himself as 'half-way': neither rich nor poor, top nor bottom: no difference from most people. Just average.

C. Describes himself as below the middle: somewhat lower than most people: poor rather than rich: working class.

D. Response not classifiable in terms of class position.

The distribution of the four types of responses is given in Table 5.

Table 5. *Verbal judgements of own class position*

Category	Boys		Girls	
	No.	%	No.	%
A	4	(7)	16	(22)
B	23	(43)	38	(53)
C	9	(17)	5	(7)
D	18	(33)	13	(18)
A+B+C+D	54	(100)	72	(100)

Table 6. *Self-choices in relation to objective class ratings and subjective verbal judgements*

Objective class groups	Verbal judgements of own class position				Mean self-choices
	A	B	C	D	
	(a) Boys				
1-4	0	5	0	2	4.36
5	3	10	4	7	4.18
6, 7	1	8	5	9	4.45
Mean self-choices	3.62	4.62	5.11	4.12	—
	(b) Girls				
1-4	4	2	0	5	2.91
5	4	22	2	5	3.68
6, 7	8	14	3	3	3.48
Mean self-choices	1.94	3.78	4.90	3.96	—

The sex difference is again significant at the 5% level. Girls tend to place themselves at a higher position in the social hierarchy than boys. A majority of both sexes, however, describe themselves as 'in between'.¹

In Table 6 (a) and (b) both sets of data are combined. Reading from left to right, they show how mean self-choices vary according to objectively rated class groups and from top to bottom how they vary with verbal judgements made by the subjects themselves.

From Table 6 it is clear that the self-choices are, in fact, a function of subjective class feelings.

¹ This is in agreement with the findings reported by Stendler⁽⁸⁾, p. 63.

(iii) *Correction of scale values and results*

A further problem concerning the actual nature of the subjects' reference groups now arises. It cannot be assumed that the young people's conception of the arrangement of occupations along the scale, which may be called their 'ideal scale' will be identical with the Hall-Jones scale in absolute terms. In fact, all the evidence from work like Sherif's, demonstrates that the perception of a scale depends upon the perceiving subject's position on it.¹

The next step, therefore, was to probe the relation between the subjects' 'ideal scale' and the H-J gradings. This was done by using the responses to the question 'What sort of job would that be?', asked each time a scale position had been chosen by a boy or girl.

A preliminary check dealing with a variety of jobs indicated that there was a fairly close correspondence between the relative positions of jobs on the subjects' 'ideal scale' and the H-J scale. The types of job included under the H-J value of 5 were therefore used in attempting to establish a more exact relation. The jobs in this group (skilled manual and the routine grades of non-manual) were mentioned by most subjects at some stage in the course of the scale test. The mean scale values in fact assigned were 4.12 for boys and 3.72 for girls.

It thus appears that the subjects' ideal scales exhibit a systematic upward displacement, i.e. a shift such that their own position is situated higher up on the ideal scale than it is on the H-J scale, the shift being greater for girls than for boys.

Mean scale values assigned by the boys and girls to skilled-manual and routine non-manual work can now be used for bringing the original self-choice values into line with H-J grades. The adjustment is, of course, a rough one, as it assumes linearity over the whole range, which is only crudely approximated; but for the purpose of providing a view of the general pattern the distortions are probably not serious.

The corrections were:

$$5.00 - 4.12 = +0.88 \text{ for boys,}$$

and

$$5.00 - 3.72 = +1.28 \text{ for girls.}$$

These corrections, applied to the figures in Table 4, gave the final result of the self-choices on the class scale.

(iv) *Summary of class attitudes*

The findings expressed in Table 7 will now be outlined. The modal reference group for all objective class groups (1-7), indicated by the distribution of corrected self-choices, fell within the socio-economic class range covered by H-J 5 ± 0.5 : namely, skilled workers, routine clerks and the better type of shop assistant. This applied to approximately 60% of all boys and 50% of all girls. A further 20% of boys and girls made their choices within the range of H-J 5 ± 1.0 , i.e. the next higher or lower grade. This leaves only 20% of the boys and 30% of the girls whose self-choices were further removed from H-J 5.

On examination of the objective class-groups a striking fact emerges: 6 and 7 tend to have a reference group higher than their actual membership group, and 1-4 a lower one. The former effect is greater for girls and the latter for boys, owing to the girls' higher mean self-choice within each objective class group. All this suggests that in spite of

¹ Cf. Marks (5), Sherif & Cantril (5).

considerable heterogeneity in externally assessed or 'objective' class status, the majority of boys and girls shared a more or less common reference group.

The question may be asked whether the existence of such a common reference group went together with an absence of class distinction in behaviour, or whether cleavage occurred corresponding to objectively rated class levels. This was investigated by means of the data derived from a sociometric device, in which the young people were requested to

Table 7. *Corrected mean self-choices*

	Boys						Girls					
	<i>a</i>	<i>b</i>	<i>c</i>	<i>d</i>	<i>e</i>	<i>f</i>	<i>a</i>	<i>b</i>	<i>c</i>	<i>d</i>	<i>e</i>	<i>f</i>
	Mean class score of group	Mean corrected self-choices	Difference <i>a-b</i>	No. of self-choices within H-J 5.00		No.	Mean class score of group	Mean corrected self-choices	Difference <i>a-b</i>	No. of self-choices within H-J 5.00		No.
				± 0.5	± 1.0					± 0.5	± 1.0	
Objective class groups												
1-4	2.86	5.24	-2.38	5	6	7	3.45	4.19	-0.74	6	7	11
5	5.00	5.06	-0.06	17	21	25	5.00	4.96	+0.04	14	23	33
6, 7	6.42	5.33	+1.09	10	18	22	6.36	4.76	+1.60	17	21	28

The boys in the objective class groups 1-4 will be used to explain Table 7. This group consists of:

- 1 boy in H-J 1: class score 1
- 2 boys in H-J 2: class score 4
- 1 boy in H-J 3: class score 3
- 3 boys in H-J 4: class score 12

7 boys in H-J 1-4 with a total class score of 20.

The mean class score for this group is therefore $\frac{20}{7} = 2.86$.

With this, set out in column *a*, the mean corrected self-choice of the group shown in column *b* can be compared. It is 5.24 and therefore 2.38 H-J grades lower. This means that the 1-4 boys identify with a social group considerably below their own, as objectively rated.

As regards the distribution of the corrected self-choices of these boys, 5 out of 7 fall within the range of half a unit (± 0.50) of H-J 5.00 and 6 out of 7 within a range of one unit (± 1.00) of that value.

The remainder of the table is to be interpreted in the same way.

Table 8. *Combined 'best friends' choices by boys and girls*

H-J groups of subjects chosen	(Independence values in brackets.) H-J groups of subjects choosing			No.
	1-4	5	6, 7	
6, 7	(7.31)	(25.17)	(21.52)	54
	8	24	22	
5	(7.31)	(25.17)	(21.52)	54
	7	27	20	
1-4	(3.38)	(11.65)	(9.96)	25
	3	11	11	
1-7	18	62	53	133

name their three best friends among the leavers. The procedure employed was to plot the objective class ratings of the choosers against those of the friends chosen. Independence values were then worked out to compare the result with chance expectation (Table 8).

Observed frequencies are very close to independence values. Social class difference therefore appeared to play only a minor part in the behaviour of these young people at school.¹

¹ This result conflicts with that of a recent Liverpool study by Silberman & Spice (8). They divided the children according to their clothing standards, however, without reference to the parents' socio-economic status. A high correlation between the two was assumed, but not demonstrated. In view of the homogeneity of their groups the assumption may have led to serious error.

III. LEVELS OF SOCIAL AND OCCUPATIONAL ASPIRATION

(i) *Introduction*

The pilot study had revealed young people's ideas about their social and occupational future to be complex and usually not easily accessible. A number of different, though converging, lines of approach therefore had to be developed.

First, in the course of a preliminary group-administered 'job-quiz' the following question was asked: Should young people be content to get the same kind of job their parents had before them, or should they try to get a higher position in the world? Over two-thirds of the boys and girls said that one should try to get a higher position.

Secondly, in the card-ranking both sexes assigned the highest mean rank to the card labelled 'GOOD CHANCES OF GETTING ON', which means that they considered this to be the most important aspect of any job. Furthermore, boys and girls throughout the interviews came out with such phrases as:

I want to get on.
I would like to better myself.
I'll work my way up.
I intend to go to the top. *Etc.*

Does all this mean that an ideal of maximum social mobility was operative in the sample, or does it merely represent the acceptance of verbal stereotypes current in a society whose values remain predominantly middle-class?

(ii) '*Autistic Aim*' on the class scale

An answer was obtained by further experimentation with the social class scale. In addition to the self-choice described in the preceding section, subjects made a choice on two more criteria. One of these may be labelled 'Autistic Aim' and the other 'Realistic Expectation'. The purpose of 'Autistic Aim' was to bring out fanciful day-dreams in order to differentiate them from goals judged to be attainable. It was introduced as follows:

Suppose you were quite free to choose whatever position you wanted to have: imagine for a moment that there are no difficulties or obstacles in getting there—you wave a magic wand and get there just by wishing: where then would you like to be?

Next came 'Realistic Expectation':

Nobody, unfortunately, has a magic wand: some of us can get the position we should like to have and some of us can't. Now show me the position where you think you will be when you are older. Where do you expect to be then?

Autistic Aim only is to be discussed in this section and results are given in Table 9.

Both means are well below the potential maximum, girls again being significantly higher than boys (P less than 0.01). This indicates that the widespread desire to 'get on' does not imply a wish really to get to the top of the social pyramid. As Table 10 illustrates, few did in fact choose the top.

Probably the number of these who went right to the top would have been smaller but for the intervention of the investigator. Boys or girls who failed to move right up spontaneously were asked: 'Are you sure you would not like to go to the top if you had the chance?' Some then went right up, possibly only to oblige. The question was necessary in view of the next step. Boys and girls who had declined to go any further were invited

Table 9. '*Autistic Aim*' choices: mean raw¹ scale values

Sex	Mean raw ¹ scale values	S.D.	Maximum possible scale value
Boys	6.06	1.60	9.00
Girls	6.83	1.36	9.00

¹ Raw: i.e. not transformed into H-J equivalents: this is because the highest value (9.00) was sometime chosen; that was the top anchoring point which, like its bottom counterpart, lies outside the range of the H-J scale.

Table 10. '*Autistic Aim*' choices: proportion of boys and girls who advanced beyond their self-choice point

Position	Boys		Girls	
	No.	%	No.	%
Advanced right to the top	6	(11)	12	(17)
Advanced part of the way but not to the top	36	(67)	44	(61)
Did not advance beyond self-choice point	12	(22)	16	(22)
Total	54	(100)	72	(100)

to explain why they would not wish to do so. Reasons offered tended to fall into four broad groups, given below with illustrations.

A. Disadvantages of being high up:

Boys: There's nothing in being rich—you only have worry.

If you're rich you get unhappy.

There's too much responsibility.

Girls: You're not free to do what you want up there.

Don't want to go too high—you have lots of responsibility.

When you get right up to the top people become frightened of you, and I wouldn't like them to be frightened of me.

B. Unpleasant characteristics of high-ups:

Boys: People up there are not so nice.

You get worse thought of you if you get on top because people think you're spivs.

People get funny when they get too high—don't bother about other people like.

Girls: People who are too high are snobbish.

People with too much money are always squabbling.

People who get too high are stuck up; they think they're better than anybody else, and don't speak to you very often.

C. My friends would reject me:

Boys: Most of my friends are about there: if I went higher they would treat me as if I didn't belong there, I should think.

If you get too high people begin to dislike you and you make enemies: nobody wants that.

Girls: Lots of people think that if you are high you're different and they wouldn't have anything to do with you.

Most of my friends wouldn't like to be my friends if I were much higher.

D. I'm happy where I am:

Boys: If you're on top people say you're posh; if you're down below they say you're in the dump.

I just like to be like the ordinary people I meet every day.

I like to live among the ordinary lads and friends, not up above.

Girls: We're just nice: I've seen people who have good jobs and a lot of money and are unhappy.

I want to be like other people and not higher than my family.

Excluding, of course, the six boys and twelve girls who did move to the top, there were four boys and eleven girls who failed to respond meaningfully in terms of the above categories. The distribution of the remainder is shown in Table 11. Young people who gave multiple responses were classified according to what seemed to be their dominant attitude.

The sex difference between the two distributions is significant at the 5% level. Far more boys than girls said quite firmly that they were content to remain workers; this was sometimes qualified by the remark that they would like to 'get on', but it appeared clearly that they had no wish to move out of their class. It is remarkable that very few exhibited any pronounced hostility towards the upper class. Paradoxically, this occurred more often with the girls, although the evidence indicates that they not only considered themselves to be higher on the social scale, but also tended to be keener on moving up themselves. The apparent contradiction is understood when one observes that many girls must have experienced a conflict—on the one hand there was the desire to rise in the social scale, on the other some knowledge of the difficulties and the fear of becoming estranged from their group. Thus in the case of several girls the alleged disadvantages of high positions as well as the unfavourable characteristics attributed to those holding them, can probably be interpreted as 'sour grape' rationalizations.¹

Table 11. *Boys and girls giving various reasons for not wanting to be at the top*

Type of reason given	Boys		Girls	
	No.	%	No.	%
A. Disadvantages of being high up	12	(27)	17	(35)
B. Unpleasant characteristics of high ups	8	(18)	17	(35)
C. My friends would reject me	7	(16)	8	(16)
D. I am happy where I am	17	(39)	7	(14)
Total	44	(100)	49	(100)

(iii) *Realistic expectations on the class scale
and verbal statements of aim*

Most young people found this more difficult than either self-choice or autistic aim. Several said they did not know where they were going to be, but agreed to have a go. Talking things over afterwards, it emerged that there were two main approaches to the task. Some boys and girls had a definite idea—in terms of social status, occupation, or both—where they expected to be, and located the point according to their ideal scale. Others, rather vaguely, thought of getting on and just pushed the pointer up, presumably in rough proportion to the extent to which they anticipated getting on. Hence, difficulties in the way of interpretation were too great to justify detailed analysis of the data. It is worth noting, however, that for nearly half the boys and girls their realistic expectations appeared to involve no change from the position in which they saw themselves on the self-choice scale.

In addition to the questions bearing on autistic and realistic aims, at a convenient stage during the interview the young people who had not previously volunteered their ideas were asked:

What are you going to be in later life?—what sort of position will you try and get when you are older?

¹ In this connexion it may be noted that the unpleasant quality most frequently attributed to "high-ups" by the girls was "snobbishness".

The questions were deliberately loaded a little in the direction of upward mobility, so as to encourage the boys and girls to talk freely of their ambitions and hopes. Allowance has to be made for this when considering the results, which should be taken to represent the upper limit of aspirations for the group. Those who did not know or had not thought were not pressed.

The answers revealed, all too clearly, the difficulties familiar to vocational psychologists. As illustrated by the quotations below, the aims expressed varied in clarity, and it was almost impossible to assess the varying intensities of desire to reach them.

(a) Clarity (boys):

Maximum: I would like to become a master printer: Then if I wanted to try something else and it didn't work, I could always fall back on that. I should like to become a foreman later but I wouldn't stand a chance of becoming a manager or owner.

Minimum: I want to come over some men and give boys advice: they call him manger or something

(b) Intensity (girls):

Maximum: If I get into nursing I'll definitely try hard to become a sister or a matron.

Minimum: When I'm older I'd like to be over two or three girls, looking after their operating, but I wouldn't worry me if I couldn't be.

For what it was worth, an attempt was made to tabulate and group the answers in accordance with the H-J grades. The class membership grades of each subject were then correlated directly with the H-J values of their aims. The correlation coefficients were +0.13 and +0.14 for boys and girls respectively—neither of them significant. There was thus no consistent relationship between *absolute* level of aspiration and class membership.

When the level of aspiration was treated *relatively*, as a rise above the subjects' present class membership, the result was different. The correlation coefficients became -0.61 and -0.55 for boys and girls respectively (for both, P less than 0.01).¹ This inverse relation means that boys and girls lower in the social scale wanted to advance relatively more (but not to reach higher positions) than those belonging to a higher class. While the difficulties of interpretation prevent any stress on this, the data as far as they go reflect the same levelling tendency as that found earlier.

IV. SUMMARY

1. An investigation of social class attitudes was carried out with a group of secondary modern school leavers, 56 boys and 77 girls. The chief method was the intensive interview. A number of specially devised techniques, among which the 'Social Class Scale' is the most relevant here, were also used.

2. Most boys, whatever their actual class membership, identified themselves with (or selected as reference group) approximately grade 5 of the Hall-Jones scale. Boys whose parents were above this grade tended to underrate their position in the social hierarchy whilst some unskilled or semi-skilled workers tended to overrate theirs.

The same was true of boys' long-term social and occupational aspirations. Expression like 'good prospects', 'a job with a future', 'a place where you can get on' were mentioned among the main desiderata of any job. This must not be misconstrued as implying that

¹ Schematic exposition of the procedures: subject N . (Class membership $H-J = X$) has expressed the aim to achieve position P ($H-J = Y$). First correlation between (Y) and (X). Second correlation between ($X - Y$) and (X).

the boys indulged in flights of fancy. A few wished to become supervisors or become independent in their trade by starting a small business. However, they did not appear to be over-anxious to achieve these aims, but regarded them as desirable possibilities. Many declared that they just wanted to become fully skilled craftsmen and, to them, this is what a job with prospects implies.

A deep sense of working-class loyalty pervades the boys' remarks. They intend to stay with 'the lads'. Boys from semi- or unskilled parents wish to become skilled workers. Sons of craftsmen are content to stay where they are: and the group ideal is accepted at least verbally, even by those whose parents are higher in the social scale.

3. In comparison with boys of equivalent parental status, girls rated themselves higher in the social scale. Their modal reference group was also grade 5 of the Hall-Jones scale, but with a greater upward scatter. Girls belonging to a lower grade overrated themselves more than boys of a corresponding class and girls of a higher grade underrated themselves far less.

Phrases like 'good prospects' and 'getting on' or 'working oneself up' have a more direct reference to upward social mobility than is the case with the boys. This does not imply that their ideal is to get to the top of the social ladder, but their aim is about the third or fourth grade of the Hall-Jones scale, which is substantially higher than most boys'. Girls wished to become private secretaries, manageresses of stores, etc., and a few soared even higher in their aspirations. Girls are more ambitious, but their ambitions are less realistic. Moreover, many girls seemed to experience a conflict between the desire to move up and loyalty towards the group to which they are attached.

The writer wishes to acknowledge the help of Miss N. Williamson in preparing and typing the manuscript, and of his wife in editing the material.

The study was supported by a grant from the Central Research Fund Committee of London University.

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(Manuscript received 3 June 1952)

BEFORE AND AFTER INDEPENDENCE A STUDY OF RACIAL AND COMMUNAL ATTITUDES IN INDIA

BY S. P. ADINARAYAN

Madras Christian College, University of Madras

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I. INTRODUCTION

In spite of the fact that during the last three centuries India has been the sphere of some very bitter racial and communal clashes, no purely psychological investigations as to the nature of the motives, attitudes and ideologies involved in these conflicts has been undertaken until very recent times.¹ The most important question investigated in the present inquiry is: Do national events of first-rate importance have any effect on the racial attitudes of the people concerned? In 1947 two events destined to have the greatest possible significance for the Indian people took place, i.e. the attainment of independence and the constitution of the separate state of Pakistan. By a lucky chance the present investigator had studied, by the Bogardus's Social Distance Test, the racial attitudes of a group of South Indian Hindus, before these two events took place. Two years after independence he gave the same test to other groups of South Indian Hindus roughly comparable in age, education and status to the first group. Thus some objective data on the possible effect of these two events on these subjects were obtained.

It is popularly believed that the Hindu is more caste conscious than the Muslim. The latter, since belief in universal brotherhood is one of the fundamental features of his faith, is supposed to be without any caste prejudices at all. But there is some evidence to show that the long contact with the Hindu has made the Muslim also caste conscious to some extent. What is his attitude to the untouchables of Hindu society? Is it more liberal than that of the Hindu, as it ought to be? Is the popular belief in the catholicity of the Muslim mind justified? Attempts are made in the present investigation to throw some light on these matters.

Are students more liberal minded than older people? It is commonly believed that the teen age is one of unrestricted idealism. In the present inquiry a comparative study is made of the racial attitudes of Hindu and Muslim students as opposed to those of Hindu and Muslim workers. A comparison is also made of the racial attitudes of men and women.

Historical circumstances were favourable for the formation of a more liberal attitude towards the Muslim in the South of India than in the North. This investigation seeks to ascertain experimentally whether this favourable attitude still exists or has been transformed by the course taken by later history.

¹ The results of these recent studies have not yet been published and are awaited with interest. There have been of course many excellent historical, economic and sociological surveys of racial and communal problems in this country and some of them make excursions into the psychological field. For example Mr Roy Chowdry's analysis of the concept of Pakistan. Mr W. S. Taylor⁽²⁾ has made some interesting studies of religious attitudes in India.

II. METHOD

The investigation took place in two stages, and for the sake of convenience the first stage will be referred to as the pre-independence investigation and the second stage as the post-independence investigation. In both investigations Bogardus's Social Distance Test and a qualitative questionnaire were used.¹ The pre-independence investigation took place during the years 1934-45 and the post-independence one during the year 1950.

In both the investigations the questionnaire was well received. Most of the people approached showed great interest in the project and often asked for more copies to be distributed among their friends. For the first investigation 1000 copies of the questionnaires were printed, of which 550 were distributed among Hindus, 200 among Muslims and 60 among Hindu and Muslim women. Of these 340, 105 and 40 respectively were returned, thus making a total of 485 returned out of 810. Out of this 258 were filled in anonymously. Loss of questionnaires occurred most when bundles of them were sent to a single person. Copies given personally by the investigator to individuals were invariably returned. The nature of the answers given in the second questionnaire of the post-independence investigation, as well as the comments written at the end of questionnaires in both investigations, revealed a high degree of co-operation on the part of the subjects.

In the post-independence investigation 150 mimeographed copies of the questionnaire were distributed, out of which 105 were returned; 50 people chose to be anonymous.

The following general instructions were given on the front page of the questionnaire booklet:

*Confidential**Important*

Read this carefully before proceeding further. An investigation is being conducted into the psychological factors which affect race attitudes, with special reference to conditions in India. The investigator, who holds a responsible position in an educational institution, seeks your co-operation in the investigation and gives his personal guarantee that the information supplied by you will be kept strictly confidential and will not be used for political or propagandist purposes but solely in the interests of scientific research. Your identity, if you choose to give your name, will not be revealed under any circumstances whatever, but you need not give your name if you do not want to. The object of this inquiry is not to judge your moral conduct but to ascertain your actual views and attitudes. You are therefore requested to give only your personal opinions and not allow your answers to be affected by political or moral considerations or by the opinions of others. Please be absolutely frank in your replies, concealing nothing either because of your own feelings or those which you may attribute to the investigator.

Now turn over and answer the questions in the following pages as accurately as possible. In the event of your answering the questions or as many of them as you can and returning the questionnaire you will be rendering a great service to the investigator.

Please fill in the following detail:

1. Name (optional).
2. Sex and age.
3. Occupation.

Bogardus's Social Distance Test. This test was given in both the inquiries. The test as re-adapted for use in these ran as follows:

Directions. Underline the word which expresses, or most nearly expresses, the way you feel towards the members of other groups, nationalities, or races (as a class, and not the best members you have known, nor the worst) with regard to certain relationships stated below.

¹ The results of the qualitative investigation have not been included in this article.

Example: according to my first feeling reaction, I would willingly admit: Any: Most: Some: Few: No—Americans to live and work in my country.

1. According to my first feeling reaction, I would willingly admit:

- (a) Any: Most: Some: Few: No—Canadians as visitors in my country.
- (b) Any: Most: Some: Few: No—Canadians to live and work in my country.
- (c) Any: Most: Some: Few: No—Canadians to full citizenship including the right to vote in my country.
- (d) Any: Most: Some: Few: No—Canadians to my school or University, to my profession or occupation.
- (e) Any: Most: Some: Few: No—Canadians to my club.
- (f) Any: Most: Some: Few: No—Canadians to my home as my personal friends.
- (g) Any: Most: Some: Few: No—Canadians to close kinship by marriage.

The same question was asked regarding Turks, Belgians, British, Chinese, Americans, Japanese, French, Burmese, Negroes, Harijans and Muslims.

The questionnaires were marked by the investigator throughout. The subject received a plus mark for 'Any' and 'Most' and a minus mark for 'Few' and 'No'. 'Some' being a neutral attitude was not marked at all. The number of marks given varied according to the nature and degree of the statement endorsed. Thus, for example, willingness to have any members of a nation as visitors received only 2 marks as this did not involve a great amount of racial friendliness, whereas unwillingness to admit any member of a nation as a visitor received a large minus mark (-14) as this indicated the presence of definite racial hostility. Conversely, willingness to receive members of another nation into close kinship by marriage received a very high plus mark ($+14$) as this indicated a high degree of racial friendliness, whereas unwillingness in this matter received only a small minus mark (-2) as it was felt that cultural, economic and social considerations besides that of race entered into the question of marriage. The scheme of marking used is shown in Table 1.

Table 1

	Any	Most	Some	Few	No
As visitors	+2	+1	0	-7	-14
To live and work	+4	+2	0	-6	-12
To full citizenship	+6	+3	0	-5	-10
To School, University, profession or occupation	+8	+4	0	-4	-8
To one's club	+10	+5	0	-3	-6
To one's home	+12	+6	0	-2	-4
To close kinship by marriage	+14	+7	0	-1	-2

III. THE SUBJECTS

Pre-independence investigation. The 485 subjects taking part in this investigation were mainly drawn from the United Provinces and the Madras Presidency. They were confined to the two big communities of India and to the student and professional classes. They could be divided into the following seven main groups:

Hindu students (South)	75
Hindu students (North)	145
Muslim students	70
Hindu professionals (South)	45
Hindu professionals (North)	75
Muslim professionals	35
Women, Hindu and Muslim	40
Total	485

Distribution of subjects according to age

18-20	210
21-30	200
31-40	44
41-50	23
51-60	7
61-70	1
Total	485

Post-independence investigation. The 105 subjects in this investigation were drawn from the Madras Presidency, mainly from the city of Madras. They fell into the following two groups:

Students (South)	60
Professionals (South)	45
Total	105

Distribution according to age

18-20	50
21-30	30
31-40	10
41-50	10
51-60	5
Total	105

IV. RESULTS

Table 2. *Before and after independence. The marks and ranking given by Hindu students and professionals in the Social Distance Test for the ten nations*

Nationalities	Hindu students		Hindu students		Hindu professionals		Hindu professionals	
	before	Rank	after	Rank	before	Rank	after	Rank
Canadians	- 11	5	- 1.7	5	- 4.1	4	+ 0.50	5
Turks	- 16.3	10	- 8.8	10	- 13.9	10	- 6.2	10
Belgians	- 12.8	6	- 5.9	8	- 9.4	7	- 1.5	7
Chinese	- 5.1	2	- 1.0	3	- 4.2	5	0	6
Americans	- 7.6	3	+ 1.4	1	- 4.7	6	+ 3.8	2
Japanese	- 15.2	9	- 1.3	4	- 11.6	9	- 2.2	8
French	- 9.5	4	- 3.8	6	- 3.1	2	+ 2.8	3
Burmese	- 1.8	1	- 8.3	9	- 2.4	1	- 3.7	9
British	- 13.2	7	- 0.56	2	- 3.9	3	+ 5.0	1
Negroes	- 13.7	8	- 4.7	7	- 11.2	8	+ 1.0	4

Table 3. *Pre-independence investigation. Comparison between the average marks of the two sexes in Bogardus's Social Distance Test*

	Number	Average score	Standard deviation
Men	446	+ 97.233	15.36
Women	40	+ 95.65	12.48
Difference between averages		1.583	
Standard error of difference		2.1	

Difference between averages is considerably less than twice the standard error; hence there is no significant difference between the average scores.

Table 4. *Pre-independence. Attitude of the Hindus of the South and the North towards Muslims*

Hindu students (South)	+ 13.7
Hindu students (North)	- 1.1
Hindu professionals (South)	+ 15.4
Hindu professionals (North)	+ 4.7

Table 5. *Pre-independence. Attitude of Hindu students and Hindu professionals towards Muslims*

	Score
Hindu students	+ 3.5
Hindu professionals	+ 8.7

Table 6. *Pre-independence. Attitude of Muslim students and professionals towards the Hindus*

	Score
Muslim students	- 1.0
Muslim professionals	+ 14.7

Table 7. *Pre-independence. Difference in the attitude between Hindus and Muslims towards Harijans¹*

	Score
Hindu students	+ 19.2
Muslim students	+ 1.2
Hindu professionals	+ 21.3
Muslim professionals	+ 9.9

Table 8. *Pre-independence. Difference in the attitude between Hindus, Muslims and Women towards Harijans*

	Score
Hindus	+ 20
Muslims	+ 4
Women (Hindu and Muslim)	+ 20.8

Table 9. *Difference in the attitude towards the Muslim by the Hindu students and professionals (South) before and after the creation of Pakistan*

	Score	
	Before Pakistan	After Pakistan
Hindu students (South)	+ 13.7	+ 8.1
Hindu professionals (South)	+ 15.4	+ 7.4

V. CONCLUSIONS

The research produced results and data of some importance to the social psychologist. There is much loose talk about feelings and attitudes in India. Interested people are apt to attribute to the public attitudes which either they think that they ought to possess or they wish they did. An exact study (though in a very limited way) under standard conditions of what the people are thinking is, therefore, some corrective to loose thinking. To know something specific even about a few is better than guessing about many.

¹ Harijans—literally people of God—is the official name now given to the 51,000,000 of India's population who have not been assimilated into the caste system and on this account suffered many social and cultural handicaps in the past. Recently there has been a nation-wide awakening about their condition and many attempts are now being made to up-grade them.

Table 2 contains the answer to one of the most important questions asked in the present inquiry—does a subject nation change its international outlook on becoming independent? More specifically does its attitude towards its erstwhile rulers undergo any change? This table gives the racial attitudes of Hindu students and Hindu professionals as gauged by the Social Distance Test given to roughly comparable groups before and after Independence. There is no marked change as regards the Turks who occupy the last position in the student and professional ratings, both before and after Independence. That of the Canadians is also roughly parallel and the same applies to the Belgians. The case of the Chinese is however striking, inasmuch as Hindu students in both the inquiries tend to rank them much higher than do the Hindu professional workers. This may be taken as an indication that the change that has been gradually coming over the Chinese government (which roughly coincides with the period of our inquiry) does not meet with the approval of the Hindu professional classes while it leaves the Hindu student unaffected. Americans gain after Independence in both groups going from 3 to 1 as regards students and from 6 to 2 as regards professionals. After Independence there has been much diplomatic relationship on a footing of equality between the two countries which was spotlighted by the fine reception given to the Indian Premier when he visited America. All this has tended to build up a pro-American sentiment in India of which the present ranking is an indication.¹

Another striking feature is that of Burma losing ground both with students and professionals after Independence, having been demoted from 1 to 9 in both cases. Why has free India turned against Burma while subject India was her best friend? Has a sense of rivalry crept into their relationship now that both are independent and no longer present a united front to a foreign ruler? Perhaps it is a reply to a growing anti-Indian feeling in Burma itself.

Negroes continue to be out of favour both before and after Independence except that they gain four places with the Hindu professionals after Independence. This is an indication that colour prejudice is not always a matter of two contrasting colours but also that of gradations within the same colour.

We now come to the important question—what is the attitude of the Indian towards the British now that he is free from political domination? At once we come across an important fact. Among the students the rank of the British has gone up from 7 to 2 while they also gain upon the already favourable position they held with the professionals and actually become the most favoured nation. This seems to suggest that bitter memories do not linger and with a radical change in relationships one starts with a clean sheet, as it were.

The second big division of our inquiry dealt with the attitudes of Hindus and Muslims towards each other and of both towards the Harijans. Reference has already been made to the more favourable atmosphere for Hindu-Muslim relationships prevailing in the South of India as opposed to the North, and also to the popular belief that the Muslims are better disposed towards the Harijans than the Hindus. Tables 4-6 deal with Hindu-Muslim attitudes. A glance at the scores in Table 4 at once reveals the fact that Hindu students (South) give a score of +13.7 to Muslims, whereas Hindu students (North) give a score of -1.1. In the case of Hindu professionals the contrast is not so glaring but even here it will be noted that there is a difference of +10.7 between North and South (in

¹ There has been a set-back to this by the political developments in 1951.

favour of South). Both sets of figures thus confirm the general belief prevalent in India that Hindu-Muslim tensions are less in the South than in the North.

Table 5 treats Hindu students and professionals as two big groups instead of subdividing them into North and South. It is somewhat surprising to find that the Hindu student is less favourably disposed towards the Muslim than the Hindu professional. Youth is considered a time of idealism and of liberal attitudes and one would have expected the Hindu student to be more tolerant towards the Muslim than the Hindu professional. But probably student attitudes are more theoretical in nature and tend to mellow with practical experience. Our conclusion about relatively high racial bitterness among students in India is confirmed by the findings in Table 6 which gives the attitude of Muslim students and professionals towards Hindus. The Muslim student's attitude towards the Hindu is expressed by a minus quality while that of the professional worker is a fairly high plus.

Table 7 deals with the attitude of Hindus and Muslims towards Harijans. Here our findings are directly opposed to popular belief. Comparing the scores obtained by Hindu and Muslim students as regards Harijans we find that the Hindus score +18 points more than the Muslims. A similar high difference is found in the score of Hindu and Muslim professionals. Of late much thought has been given to the fate of the Harijan and perhaps the Hindu is making a conscious effort to be friendly whereas the Muslim takes his catholicity for granted.

Table 9 contains the hint of an answer to another important question asked in the present inquiry—has the creation of Pakistan made any difference to the attitude of the Hindu student and professional of the South towards the Muslim? This attitude continues to be a favourable one as the four scores given are all plus quantities. But the Hindu student's score goes down from +13.7 to +8.1 and the Hindu professional's from +15.4 to +7.4.

In the present inquiry an attempt has also been made to see whether there is any significant difference in the racial attitudes of men and women in India. The results are given in Tables 3 and 8 and are on the whole negative. The difference between the average marks obtained by men and women in the Social Distance Test is very little and statistically insignificant. The same applies to scores obtained by men and women as regards Harijans.

VI. SUMMARY

Subject to the limitations of the size and method of inquiry, and these must be stressed our main conclusions may be summarized as follows:

1. That the attainment of Independence by the Indians has brought about a change for the better in their attitude towards Britain.
2. The South Indian Hindu has less racial ill-feeling towards the Muslim than the North Indian Hindu.
3. That the popular belief that Muslims are less caste-conscious than the Hindus does not seem to be true as regards their attitude towards the Harijans.
4. That the attitude of the Hindu towards the Muslim has undergone a change for the worse after the creation of Pakistan but that this change is not big enough to be described as a radical one.
5. That there seems to be little significant difference between the racial and communal attitudes of men and women in India.

ACKNOWLEDGEMENTS

To all those subjects who answered my questionnaires and to my many friends all over India who helped me in the distribution of them I wish to express my great indebtedness. My grateful thanks are also due to Dr J. C. Flugel of University College, London, and Dr G. Murphy of the City College, New York, for much help and encouragement.

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(*Manuscript received 8 May 1952*)

PERCEPTION AND UNDERSTANDING OF INSTRUCTIONAL TELEVISION PROGRAMMES

By M. D. VERNON

University of Reading

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I. INTRODUCTION

It is clear that television is now one of the most important among the many forms of presenting ideas and information by a combination of visual and verbal methods. Though in certain respects there is considerable resemblance between instructional television programmes and documentary films, yet there are also certain important differences. The television programme suffers from severe temporal and spatial limitations which will be discussed below. But it can be immediately topical, and can apparently create in the viewer an 'impression of reality', which is extremely striking, and which may go far to make up for its other limitations.

The protagonists of 'visual education' have often cited television as being one of its most valuable methods. But in fact little seems to be known as to what are the actual impressions obtained by the viewer from adult television programmes. And there is considerable doubt as to the most valuable features of these programmes and the most effective way of constructing them.

Through the kind co-operation of the Audience Research department of the B.B.C. the author of this paper was enabled to make some pilot studies of the reactions to certain representative instructional television programmes. The results obtained were indicative rather than conclusive, since the number of subjects was small, and it was not possible to exercise close experimental control. Yet these results were on the whole consistent with one another, and with what is known already about visual instruction. Thus it seemed they might be of some value to those interested in 'visual education', and indeed in visual perception and cognition generally.

II. METHOD OF EXPERIMENT

(i) *Procedure*

The general method of experiment was to invite small groups of not more than ten or eleven subjects to observe one or more television programmes, instructing them that they would be asked immediately after the completion of the programme to write down a brief account of what they remembered. They were told: 'Imagine that some one asked you what the programme was about, and this is what you told them.' After the subjects had written their reports, they were asked questions as to what features of the programme had especially interested and impressed them, or which had bored or confused them; and there was some general discussion of these points.

There were three series of programmes which were studied:

- A. Five half-hour talks on modern building and town planning.
- B. Two half-hour talks on contemporary political and international affairs.
- C. One 40-minute talk on the effects of extreme heat and cold on the human body.

Telefilms of three of the series A programmes were made available, and were presented to other groups of subjects. They were projected on a small screen, the same size as the television screen, and thus closely resembled television presentations.

The series A programmes were seen by groups of: (a) W.E.A. students; (b) R.A.F. men; (c) girls from the sixth forms of grammar schools. Group (a) were almost all women, aged about 40 years, who had received a grammar school but not a University education. Group (b) were young men attached to Flying Training Command, engaged mainly in administrative and orderly duties; some of them had had a grammar-school education. The series B and C programmes were seen by group (d) University students, male and female, mainly reading psychology.

In all, seventy-six reports were obtained on series A, twelve on series B and eleven on series C.

(ii) *Treatment of results*

The telediphone records of the programmes were broken up into what appeared to be the principal points contained in the programme; and each subject was scored for each point remembered. But it did not seem that numerical scores of points remembered would have high validity for so small a number of subjects for each programme. Thus the total score sheet was studied to detect which of the points were recalled by most subjects—especially those which were of crucial importance to the main argument—and which were omitted by most subjects. In this way it was possible to decide whether the main line of argument had been grasped, or only certain outstanding features; and what type of detail had been retained by the various groups who saw the programmes. Finally, the answers to the questions were studied. It was often possible to judge from these why certain features had been omitted or emphasized; and why certain mistakes and confusions had occurred.

It must of course be admitted that the results obtained did not show what the subjects would remember of the programmes over a period of time. This would have been difficult to determine in practice. It would have been affected by many fortuitous circumstances—for instance, whether the subjects were sufficiently interested to think about the programmes, discuss them or follow them up by further study. But the immediate recalls did give an indication of what impressed the subjects at the time, and what they understood. Only what stood out clearly in their minds immediately after the programme finished was likely to be retained for any length of time, and to affect their knowledge and ideas subsequently. It is, of course, true that because they expected to have to make the immediate recall, many subjects took in and retained details which they might not otherwise have noticed or remembered. Thus the results described indicate the maximum effect which could be expected from such programmes. And such omissions and mistakes as occurred in the recalls might in normal circumstances be greater.

An advantage of immediate recall was that the subject was often able to indicate specific points of confusion and lack of clarity in the programme which made it difficult for him to follow the line of argument; these might have been forgotten later. A further criticism of the method adopted might be that he could remember more than he could

reproduce in writing; and that had a questionnaire been used, these recollections would have become evident. It would, however, have been inadvisable not to obtain any spontaneous written recalls, since the mistakes, confusions and inconsistencies in these are the best evidence of lack of understanding and grasp of the content of the programme. Possibly, interesting data might have been obtained by giving a questionnaire in addition to obtaining spontaneous recalls. Two practical difficulties made this impossible: (1) the experimenter did not know beforehand what the content of the programme would be, and therefore could not prepare a definitive questionnaire; (2) each experiment would have become unendurably lengthy (as it was, each one took the best part of $1\frac{1}{2}$ hours).

III. RESULTS OF EXPERIMENTS

(i) *General recall*

The main body of results was obtained from the series A programmes, and the main conclusions must be drawn from these. Certain interesting and important points, however, appeared in reports on series B and C, which are also worth consideration. The information presented in the series A programmes was straightforward, factual and easy to understand, and there was little generalized argument. Any confusion in the impressions obtained was due mainly to the actual methods of presentation and not to any difficulty in the content. In general, the reports made on these programmes by the subjects of groups (a) and (c) were accurate, and followed fairly closely the sequence of the programme. Group (a) recalled 75% of the main points accurately. Group (c) recalled 60%; they tended to omit certain sections of the programme, but their reports were coherent and accurate as far as they went. There was little distortion in these reports, and it appeared that the 'reconstructive' processes of remembering had operated to only a small extent. This gives an indication of the maximum amount which can be assimilated and reproduced in immediate recall by educated and intelligent individuals who are attending closely, with intention to remember.

Some of the reports of group (b) gave an indication of the impressions which the average viewer might be expected to retain. The over-all accuracy of recall of the main points was 51%, and some of the subjects wrote reports comparable to those of group (a). But others merely produced a general impression of the whole programme, containing some main points, but lacking in detail, and often showing the distortions which are characteristic of 'reconstructive' memory. Thus the poorest of the reports, on the building of modern schools, ran as follows:

The schools of today have a better outlook than they did in old days. They have better times than they did in 1850. There are more school today and they are putting up more. In old days the rich had the best school to go to the education of the poor was so bad.

(Incidentally, the programme did not refer in any way to the schooling of the rich.) Such men did not find their task interesting, did not attend closely, and merely picked out a few points, stating them in a context which originated in their own preconceived opinions.

Thus it appears that if a programme has little interest for a particular viewer, and if he feels under no obligation to remember it, the same types of confused and distorted impression may be obtained as were noted by the author in studies of the retention of verbal and graphical material presented by other methods (see Vernon (3)-(6)). The particular appeal of television does not automatically produce accurate impressions. Indeed, it will appear from the subsequent discussion that even if it is made interesting to the viewer, certain

other qualities are also necessary to ensure a real understanding of the programme as a whole.

The programmes of series B and C were less simple and straightforward than those of series A; those of series B contained more discussion, argument and abstract ideas. However, the subjects of group (d) were more intelligent, on the whole, and better educated, and they found these programmes interesting. Thus the main points of the series B programmes were remembered in 70% of cases; and those of the series C programme in 76% of cases. With certain exceptions which we shall describe below, there was comparatively little error or distortion in their recollections.

(ii) *Factors related to interest and impressiveness*

We have already noted that a very important factor in arousing interest was the 'impression of reality' created by the television programme. Upon what, then, was this impression based? In some cases, in live broadcasts for instance of sporting events, the programme does present situations which are actually occurring in 'real life' at that moment. The programmes of these series did not contain incidents of this kind. But a considerable impression was produced by one unrehearsed 'real life' incident which occurred in one of the series B programmes. It had been intended to include a telephone conversation with a newspaper correspondent in Moscow, but it proved impossible to establish telephone communication with him at the time of the programme. This breakdown of communication with Moscow was contrasted with the extreme ease of establishing transatlantic telephone communication (in the same programme), and gave the subjects a convincing demonstration of the difficulties of contacting Russia.

But though the programmes did not for the most part contain incidents occurring at the moment outside the studio, there were nevertheless in the series A programmes several film excerpts depicting naturally interesting events which had occurred in contemporary life (for instance, scenes of the 'blitz'), which were known to have 'really' happened. However, it did not seem that the 'impression of reality' was based only upon the viewer's knowledge that something of the kind had actually happened, but was affected also by other factors.

Now Michotte(2) has argued that our 'impressions of reality' can be distinguished from our knowledge of what is 'real'. If this is true, it is understandable that the 'impression of reality' produced by television is not determined solely by the knowledge of the subject that a 'real' event is being televised at that moment. Thus a televised film of earlier events may appear more 'real' than a televised studio scene of simultaneous events. Michotte has suggested that the 'impression of reality' depends primarily on the perception of the three-dimensional organization of a figure, and that it does not appear to belong to a flat plane. In particular, the appearance of three-dimensional movement liberates a figure from the two-dimensional plane. Thus in a film which is suddenly stopped, the images lose the 'appearance of reality' which they previously possessed, and seem to be located in the plane of the screen.

From the subjects' answers to questions about the strikingness and impressiveness of various parts of the programmes, it appeared that the best 'impressions of reality' did indeed arise where there was considerable three-dimensional movement—that is to say, in the film excerpts, and especially in those of people moving to and fro in natural surroundings. Thus familiarity of setting was also a pre-disposing factor. But static scenes,

whether filmed or televised in the studio, were less convincing, less lively, less impressive. In fact, it seemed that if interest was to be maintained by the visual material, some movement, some impression of people actively doing things, was almost essential. If the chief object of the programme was to arouse interest in simple factual incidents—as for instance in a news broadcast—then the film technique provided all that was necessary. But if further conclusions or inferences were to be drawn from the facts presented, then this technique was not altogether successful. The very vividness of the material caused it to stand out clearly in the subject's mind, so that he remembered only the single incidents and was unable to give much attention to other parts of the programme, and particularly to any generalizations based upon the specific incidents. However, we shall return to this point later.

Whereas the film excerpts appealed readily, relatively static studio scenes were usually less interesting; and still pictures, even less. Some 'impression of reality' and liveliness was given in the former by the actions of the speakers in the programme. Thus in the series C programme, the speakers performed a number of simple scientific experiments and demonstrations which in general were striking and impressive—especially those involving a good deal of human activity. Again, expressive movements, gestures and facial expressions all enhanced the impression of real living people behaving in a natural manner. Contrasted with this, was the observation made by a number of subjects that stiff unnatural groupings of speakers who seldom moved about had a deadening effect, and made them seem boring and unimpressive. A further drawback was that the small size and lack of clear definition of the television images made it hard to perceive facial expressions except in close-up view. It is also difficult, of course, for the television camera to cover the movements of people over any but a narrow area. Sometimes this is accomplished by rotating, or 'panning', the camera to follow them as they move. But there is a danger that, since their images remain in the same position on the television screen, the background may appear to move as its image moves across the screen. This apparent movement of the background may be disconcerting.

The programmes in series A included several demonstrations of models of buildings and of their lay-out. These were of two kinds: (1) realistic three-dimensional models which resembled real buildings of houses, shops, factories, etc.; (2) flat two-dimensional models, like jig-saw puzzles, which were used to demonstrate the lay-out of buildings, and which were diagrammatic rather than representational. The second class of model will be considered later, together with other diagrammatic material. As regards the first class, it seemed that a convincing 'impression of reality' was obtained whenever such models were televised standing alone, without any visible studio background. Indeed, it was sometimes difficult to distinguish them from real buildings being televised. Again, overhead views of such models closely resembled aerial views of towns. But if any part of the studio background became visible, or if one of the speakers was seen pointing to a model, then it immediately lost its appearance of reality and looked like a toy. Moreover, because of its small size and lack of clear definition, it was often difficult to perceive detailed structure, and sometimes quite hard to make out what the model was intended to represent. Thus, although actual three-dimensionality assisted in producing an 'impression of reality', it did not do this very effectively when the miniature size of the model was obvious.

Least convincing and impressive of the visual material were still photographs and

pictures. Their complete lack of movement made them appear 'dead' and quite unimpressive, as Michotte noted with the motionless film. And again, the small size and lack of definition of the television image often made it difficult to see what the pictures represented. When they moved, and all parts of their outlines moved together, 'closure' occurred, and the outlined image became 'figural'; thus blurring of the outline was no longer noticeable. When there was no movement, the outline ceased to be delimited from the 'ground'. Strong brightness contrast between 'figure' and 'ground' was sometimes effective in making the 'figure' stand out. Even more effective was continuity of pattern within the 'figure' (or what Gibson (1) has called 'texture'), as contrasted with a different continuous pattern in the 'ground'. Thus a 'figure' with a good deal of interior detail stood out better than a featureless surface; a mottled background showed up the 'figure' better than did a plain one.

Particularly interesting were the relationships between the 'impression of reality' and perceptibility, and the tendency towards 'size constancy'. Now with a cinema film projected on a full-sized screen, the 'real size' of the screen as compared with its surroundings is considerable, no matter how small the visual angle it subtends at the viewer's eye. Thus the images on it are in general seen as possessing the natural sizes of the objects depicted. But the size of the television screen can be seen to be small, even though the angle it subtends at the eye is fairly large (since the viewer sits near to the screen). Thus the images tend to look much smaller than the natural sizes of the objects depicted, at least to the unpractised viewer. But after a certain amount of experience of television, the viewer normally ceases to be aware of the diminutive size of the images, provided that they represent familiar real objects, such as people. (This effect of experience was shown in a subsidiary experiment with some University students; and Langdon of Oxford has made the same observation.) But this compensation did not take place readily with unfamiliar objects, the 'real size' of which was not known. Their unknown size and the poor definition of their detailed structure made them appear strange, unreal, even unidentifiable. This was noticeable with some of the three-dimensional models of less familiar appearance, as well as with the still pictures. On one occasion, one of these models, of the interior of a building seen from one side, actually appeared in reversed perspective, convex instead of concave.

If, however, telefilms of these programmes were projected on a full-sized cinema screen, the visual material, and particularly the models, immediately appeared more 'real' and more impressive. Thus clearly the actual 'reality' and three-dimensionality of the objects televised cannot in itself produce a strong 'impression of reality' from unfamiliar objects which are immobile, of small and unknown size and lacking in structural differentiation. Cinema projection increases the size and probably improves the definition; and the 'impression of reality' is enhanced.

In all these programmes, considerable use was made of 'symbolic' material, maps, charts, diagrams, and the flat models like jig-saw puzzles, already mentioned. Clearly the 'meaning' of these was constituted by what they symbolized, and was not based upon any resemblance to familiar 'real' objects. It might be concluded that such material would be lacking both in interest and also in perceptibility. But this was not the case; the material was found to be interesting and impressive whenever it presented facts clearly and simply. It was almost always easily perceptible, since its structure was simple and clearly defined, with strong brightness contrasts. Moreover, in some cases movement

was introduced, and again this enhanced the interest. The flat jig-saw models were often taken to pieces or fitted together by the speakers, to illustrate, for instance, reconstruction in town planning; and they were usually most effective. Again, diagrams were made to move and grow by drawing them on the further side of a translucent screen; the blocks or lines of the diagram, televised from in front of the screen, then appeared to move into place. This readily attracted the subject's attention; but it was sometimes difficult for him to understand what the diagram represented, until it was completed; and when it was completed, the movement of course ceased. Thus the knowledge gained was not always commensurate with the interest aroused.

(iii) *Factors related to understanding and remembering*

We have dealt so far with the factors in the visual material which made it impressive and interesting to the viewers. But it did not always follow that such features enabled them to understand and remember the content of the programme in general. We have already mentioned that vivid film excerpts, for instance, might focus attention unduly upon isolated incidents. When we proceed to study the cognitive assimilation of the content of the programme, it is necessary also to consider the effect of the verbal material, commentary and discussion, since what was actually retained, or at least what remained as a coherent body of knowledge, was determined to a very great extent by the nature of the verbal material. It might even be said that, though the intelligent literate adult may be persuaded by the interest of the visual material to attend to a television programme, yet his recollections are likely to be based mainly upon what has been said. What he sees may have little influence on these recollections unless it confuses them by conflicting with the verbal content.

Of course the verbal material also had considerable influence upon the interest and appeal of the programmes, as distinct from their memory value. This was particularly noticeable in one programme in which a professional commentator appeared; his skill in 'getting the programme across' was obviously superior to that of most of the amateur speakers, and several subjects expressed their appreciation of it. Again, many subjects said that they preferred speakers who were televised while speaking to the 'disembodied voice' of an unseen commentator, although the latter often spoke more clearly and comprehensibly. But it lacked the 'reality' and liveliness of people who could be seen acting and moving while they spoke.

For comprehensibility, however, something more was needed than a lively, pleasant speaker. A logically coherent verbal statement was necessary, even with the straightforward factual programmes of series A, to 'tie' the visual material together, give it a context, explain why it was being shown, and state the generalizations and conclusions to which it led. Thus the professional commentator mentioned above became confused at one point in his description of the lay-out of certain buildings; what he said did not correspond accurately to the visual material shown. And no one of the subjects was able to produce a coherent description of this lay-out. Moreover, most of them commented afterwards on the difficulty of comprehending this part of the programme. In another programme, the subjects were considerably distracted by the confusion of the speakers over the compass orientation of the buildings described. On the other hand, 90% of the subjects remembered vividly the verbal description of the appalling conditions of nineteenth-century slum housing, from the impressive commentary, although the accom-

any visual material consisted of still pictures which were very unimpressive and difficult to see. These programmes were in general successful in bringing home to the subjects simple emotionally toned situations such as the horrors of slum housing and antiquated schools, and the beauties of modern buildings designed to replace them. But there was a tendency to omit some of the more complex items—for instance, the reasons for using certain methods of planning, lay-out and building. Thus where 90 % remembered the bad conditions of the London slums, only 59 % recalled the necessity of building tall new blocks of flats to re-house those displaced by slum clearance. And whereas 83 % remembered that the new schools in a certain area were prefabricated constructions, only 19 % recalled the reason for prefabrication—the shortage of labour and of traditional building materials. Not only is it actually more difficult to grasp reasoning about cause and effect than simple concrete facts; but also it is very much harder for programme designers to choose material suitable for demonstrating such reasoning, and it tends to be relegated to the verbal commentary alone. Diagrammatic material was sometimes used effectively for that purpose, but its content was not always understood or retained. Thus moving diagrams were introduced to demonstrate how traffic congestion could be prevented in one town, by the building of a pedestrian shopping centre closed to vehicular traffic; 80 % of the subjects recalled the pedestrian shopping centre, but only 59 % related it to the traffic problem.

Important statements and generalizations essential to the main argument were also less likely to be remembered if they occurred near the end of the programme. The main points of the first part of the series C programme were remembered in 90 % of cases; those in the second part, in only 56 % of cases. In other programmes, important points presented in the last 5–6 minutes were often omitted from the recalls. Thus it seemed that the amount it was possible for the subjects to assimilate was limited, and that the most important parts of the content should be presented early in the programme.

It also appeared to be vitally important that visual and verbal material should correspond exactly. We have already mentioned an instance of confusion which arose when the commentator's description did not correspond with the plans and photographs shown. In another programme, a film of re-building was shown to which the commentator did not refer directly; only 43 % of the subjects recalled that this re-building was now taking place. Again, the more generalized the verbal statements, the more difficult is it to find visual material which is not so concrete and specific as to be inappropriate to them. This constitutes a really difficult problem in all programmes dealing with ideas and general arguments, as distinct from simple statements of fact. Thus the programme producer may be faced with two alternatives: to attempt to liven up his programme with visual material which is not fully relevant; or to confine it to simple factual statements which can be easily illustrated, and to omit a coherent and continuous logical argument co-ordinating the statements.

This latter procedure seemed to occur in the series C programme. As already mentioned, the individual scientific demonstrations shown in this programme were most impressive, and they were remembered accurately in 84 % of cases; but in the main as isolated incidents. It is true that the commentary was clear and comprehensible as far as it went. But it did not possess a real logical continuity, nor give any exposition of the physiological basis of the body's reactions to heat and cold. These reactions were presented, and recalled, as isolated events, with few generalized reasons as to why they occurred. Now previous

work on memory (as also the author's work on visual presentation (3)–(6)) has shown that isolated items are more likely to be quickly forgotten than are items co-ordinated by a continuous logical statement of cause and effect.

The series B programmes gave good examples of the use of illustrative pictorial and diagrammatic material in combination with a continuous verbal argument. The earlier parts of these programmes consisted of clear and logical expositions of certain political problems and ideas. This material was on the whole well assimilated, except in one or two cases where the statements made were themselves conflicting or doubtful. For instance, a description of the doubtful position of the Eastern countries *vis-à-vis* the rival Western and Russian power blocs was clearly recalled in only 40% of cases (contrasted with the score of 75% for all the main points together). As regards the influence of the visual material: In one programme a series of film excerpts showed, vividly, examples of the former imperial power of Britain, in contrast with the lack of power to-day. These events were seldom reported in detail but the general propositions they were designed to illustrate were remembered in 86% of cases. In contrast to this, certain film excerpts included in the second programme were not relevant to the verbal material which they accompanied, and the verbal statements were recalled in only 40% of cases.

But it might be expected that diagrammatic material would be particularly appropriate to demonstrate ideas and abstract arguments, since it can itself present relations and generalizations. Moving diagrams and maps were introduced into the series B programmes; they were found to be clear and useful by 83% of subjects. In particular, the data presented in three moving diagrams in one programme were remembered by 95% of the subjects. It must be noted, however, that these subjects were University students who were familiar with the use of diagrammatic material. It cannot be concluded without further investigation that other types of subject would understand equally well the diagrammatic representation of relationships and generalizations. But the simple charts and diagrams of series A were topographical, and did not present numerical data; they were well understood and remembered by most subjects. Thus a chart showing a factory layout was remembered by 83% of those who saw it; and a similar diagram was used very effectively to demonstrate the acoustics of a concert hall.

We have noted that the earlier parts of the series B programmes consisted of straightforward statements of political problems by a single speaker. The later parts consisted of discussions of these problems between representative speakers; and the visual material was confined to views of each speaker in turn. Each speaker expressed a personal point of view, in a highly convincing and lively manner. Thus feeling and opinion played a greater part than in the earlier half of the programme, and were liable to affect the opinions and prejudices of the viewers. It appeared that the statements of the discussion were less accurately remembered than those of the earlier parts of the programme, and were more liable to emotional distortion and 'reconstructive' memory. It is of course desirable to present different points of view on controversial questions; but it seems that the listener may be less likely to retain accurately the content of such a discussion than that of a straightforward statement presented in a non-controversial manner; and he may be more likely to colour his recollections with his own ideas, opinions and emotional prejudices.

IV. CONCLUSIONS

It has been claimed that the appeal of verbal and visual material presented together is greater than that of either used singly. But we have seen that various qualifications must be made to this statement, particularly as regards television, where it is impossible to have the elaborate rehearsal, the alterations of camera viewpoint and the 'cuts' in material which are possible in filming. Thus it is difficult to achieve a very close co-ordination of visual and verbal material, and we have seen that this is essential to good comprehension and assimilation. Mere temporal correspondence is not sufficient. Visual and verbal content must be integrated into a single meaningful whole. This is comparatively easy if the speaker is describing or discussing situations which can actually be televised at the same moment, or shown in an appropriate film. But there is a danger that the visual material may be more specific and detailed than the verbal, or may include features which distract the viewer's attention. With extremely generalized and abstract verbal material—for instance, in the presentation of ideas and opinions—pictorial material can provide only the illustrations of particular facts upon which generalizations are based; and it is probably important that the speaker(s) should comment on this. Generalized statements and relationships can sometimes be directly presented in charts and diagrams. But further study is really necessary to show to what extent these can be understood by the average audience. Whatever the type of programme, a clear, continuous and logically coherent verbal argument is essential to ensure understanding and remembering. The main points of the argument should be strongly emphasized, and presented as far as possible in the early part of the programme. A good deal of discussion has taken place as to whether arguments should be presented inductively—from the particular to the general—or deductively—from the general to the particular. Probably both procedures are advisable; a general statement of the essential points early in the programme, followed by the particular facts upon which the generalizations are based; and a final summing up reiterating the relationship of the particular facts to the general conclusions.

We have shown that the 'impression of reality' created by some types of television material is exceedingly valuable in attracting and interesting the viewer and convincing him of the truth and importance of what he sees and hears. This does not guarantee accurate recall, since emotional reactions may be set up which distort the viewer's perceptions and memories. But it seems to be the main factor in stimulating him to attend to the programme and to follow it. Television projection works at the moment under formidable difficulties—small screen size, poor definition, distortion and interference, etc. Yet it appears that the projection of actions and movements in three-dimensional space, especially those of a familiar nature, and human gestures and expressions, may nevertheless surmount these imperfections and create a spontaneous 'impression of reality'—that something interesting and important is really happening before the viewer's eyes, or did actually happen in this way no long time ago. We cannot at the moment specify the exact nature of this 'impression of reality' or define exactly the conditions of its occurrence—though clearly familiar movement is one of the most important. It does not appear to depend merely on the knowledge of reality; indeed, it does not normally occur when we actually perceive the 'real' world around us. It seems to be a function of the extent to which we are reminded of that world when many of the normal sensory data are absent. Does it perhaps indicate what happens when our experience is largely constructed from

pre-formed cognitive schemata, touched off by a minimal sensory pattern? And does mark the triumph over adverse conditions of the 'effort after meaning'?

I should like once more to express my gratitude to the various members of the Audien Research and Television Talks departments of the B.B.C. without whose assistance these experiments would not have been possible; and especially to Mr Silvey, Mr Littman, Mr Miller Jones and Mrs Wyndham Goldie. Also I should like to thank all those who acted as subjects in the experiments, and the head mistresses and R.A.F. personnel who arranged for the attendance of the schoolgirl and R.A.F. subjects.

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(*Manuscript received 2 October 1952*)

THE IMPRESSION OF DEPTH IN PROJECTED PHOTOGRAPHS

By E. STAFFORD AND C. E. M. HANSEL

University of Manchester

I. *Introduction to the problem* (pp. 127–128). II. *Experimental method used* (pp. 128–129). III. *Experimental results* (p. 129). IV. *Conclusions* (pp. 129–131). *Reference* (p. 131).

I. INTRODUCTION TO THE PROBLEM

Of recent years there has been a great expansion in the provision and use of visual-aid apparatus in educational institutions of all kinds, to say nothing of the increase in television viewing.¹ Such apparatus usually involves the projection of photographed material on to a screen—a two-dimensional surface. Among the many problems which require examination are those concerned with the efficiency of teaching techniques which involve the use of such material and those relating to the ease of recognition of the material thrown on to the screen. An important ‘recognition’ problem is the perception of depth in photographs which are essentially two-dimensional but portray three-dimensional objects.

One of us (Stafford) has been concerned with ‘recognition’ problems of various kinds relating to the use of visual aids in teaching. His own experience and the result of some experiments with teachers and a group of third-year psychology students seemed to indicate that as well as angle of vision, ‘screen size’, and ‘viewing distance’ are important factors.

Though a constant angle of vision is given by related variations of screen size and screen distance, variation of screen distance, keeping the visual angle constant, does not give a constant condition of viewing, as effectiveness of binocular depth perception is also a function of viewing distance.

Various experimental procedures have been devised to evaluate the effect of the factors, but it was early realized that one experiment at least would need to be devoted to the measurement of variation of depth effects with variation of viewing distance. This experiment is the subject of this paper. So far as we are aware it is the only quantitative experiment on the estimation of depth in a photograph.

The binocular overlap of the two eyes at rest is approximately 120° , but this supplies no recognition of detail except for a very small central area. Detail must be supplied by motor scanning of the display. The extent of habitual or normal binocular scanning does not exceed $35\text{--}40^\circ$ (1). Hence objects (or screens) which subtend an angle of $35\text{--}40^\circ$ or less to the eye (angle of vision) will satisfy the conditions of normal viewing. Objects (or screens) of greater angle of vision than this cause head-muscle movements in addition to eye-muscle movements and increased liability to fatigue. From the psychological point of view it may well be that there is greater realism in pictures or screen sizes which subtend an angle of $35\text{--}40^\circ$ to the eye, and some experiments one of us has made tend to confirm this.

¹ The problem of screen size and television viewing was discussed by Prof. T. H. Pear as early as 1936, in an article in the *Listener*, 4, 11, 18 November 1936.

In viewing a picture on a screen, binocular (or primary) cues from both eyes indicate the flatness or lack of depth. These primary cues are offset by cues independent of binocular vision such as past experience of the subject depicted, perspective, shadow and overlap, etc. (secondary cues). These latter indicate the depth characteristics of the subject displayed on the screen. As the distance from the screen is increased, the primary binocular cues become weaker, and it may therefore be expected that depth effects should be improved. At close distances, where the primary cues are strong, there is a possibility of strain due to conflicting cues when the effort is concentrated on depth perception. In point of fact, most subjects in this experiment did complain of strain in making judgements when seated close to the screen.

There are other variables which may affect appreciation of depth, such as contrast, depth of focus in the camera, background. In the present investigation, where the interest is centred on screen distance, the other variables have been kept constant. It is known, however, that depth effects are dependent on the angle of covering power of the lens, an effect noticed in telephoto lens photography. This is due to the variation in perspective effects of the different lenses. The angle of covering power of a normal lens, e.g. 2 in. Elmar of the Leica, is approximately 40° (when estimated from the horizontal distance and not the diagonal of the negative), i.e. the perspective effect is similar to that of the human eye. A 4 in. lens such as the Trinol has a visual angle of about 20° and hence different perspective lines for a photograph of the same size. Hence photographs of the same size of the same object, taken by two different lenses, might be expected to show a difference in depth effect (when the angle of view of the projected picture is kept constant). The focal length of the projection lens is not in itself important, as it affects the size of the picture and not the perspective.

Two hypotheses have been investigated in these experiments:

(1) That depth effects of any object depicted on the screen will decrease as the subject approaches the screen to distances where binocular cues are active.

(2) That depth effects will be decreased by decreasing the angle of covering power of the camera lens.

II. EXPERIMENTAL METHOD USED

A series of twelve wooden blocks was made of square cross-section $2\frac{3}{4}$ in. side, varying in depth from $\frac{1}{2}$ to 8 in., one of which was a perfect cube. The blocks were photographed in order of increasing size in an oblique position to the camera with a normal 2 in. Elmar lens and then with a 4 in. Trinol to give an image of the same size image on the screen. Positive film strips were made from these and used in the experiment.

At each of the five viewing distances, 3, 6, 12, 18, 24 ft. from the screen, readings were taken using the 'limiting method' of psychophysics to determine the point of subjective equality to a cube in the two series of photographs.

Subjects were asked to mark on a stencilled form whether the object seen was a cube (=) or less deep than a cube (-), or deeper than a cube (+). The order in which different viewing distances were tested was randomized among the different subjects.

A difficulty encountered in these experiments is the possibility that a subject may recognize a particular frame owing to surface marks or scratches on the film. This was obviated to some extent by instructing the subjects to make a fresh judgement on each presentation and also by alternating the series of photographs taken with the normal

angle lens, with the series taken with the wide angle lens. Both series were viewed first in ascending and then descending order of presentation.

From the data, the following were calculated: (1) the percentage error of estimation of the depth of the cube; (2) the interval of uncertainty; (3) the movement or anticipation error.

The interval of uncertainty is dependent on the number of judgements (=) given by the subjects in a run of photographs in the same series, and may be expected to reflect the degree of difficulty experienced by the subject in making a judgement. Movement or anticipation error measures the effect of serial presentation on the subject. Where the anticipation error is positive the subject tends to make a judgement early in the series either ascending or descending.

Some subjects were retested using monocular vision as, from hypothesis (1), it may be expected that if decrease in depth effect is obtained at small viewing distances (because of binocular cues), such an effect will not be present with monocular vision.

In this experiment all subjects were seated at right angles to the screen, but group experiments have been made to discover variation of depth perception with angle of viewing to the screen.

III. EXPERIMENTAL RESULTS

The results obtained are summarized in Tables 1-3 and Figs. 1-3.

Table 1. *Variation of depth effect with viewing distance: mean value of ten subjects tested at all distances, normal and narrow angle values combined (cf. Fig. 1)*

Screen distance from subject (ft.)	3	6	12	18	24
Mean length estimated as cube (in.)	3.44	3.17	3.10	3.00	2.99
Error (estimated mean length—2.75) (in.)	0.69	0.42	0.35	0.25	0.24
% error	25.1	15.3	12.7	9.1	8.7
Interval of uncertainty (in.)	0.52	0.57	0.68	0.56	0.54

Table 2. *Relation of depth effect to viewing distance for normal and narrow angle lenses (cf. Fig. 2)*

Distance from screen (ft.)	3	6	12	18	24
% error normal angle	15.6	8.0	7.6	2.5	3.6
% error narrow angle lens	34.6	22.0	18.4	16.0	14.3

Table 3. *Relation of depth effect at variable distance to monocular and binocular regard (means of two subjects) (cf. Fig. 3)*

Distance from screen (ft.)	3	6	12	18	24
% error monocular	11.6	12.0	4.7	0.7	3.0
Interval of uncertainty	1.0	0.6	0.6	0.6	0.6
% error binocular	30.7	20.4	20.9	5.2	0.2
Interval of uncertainty	0.35	0.44	0.60	0.50	0.44

IV. CONCLUSIONS

The results appear to support both hypotheses. At 3 ft. from the screen the subjects experience considerable loss of depth, and this loss diminishes as distance from the screen increases.

For an average audience viewing photographs taken with a normal angle lens, estimation of depth becomes reasonably accurate when viewing distance is greater than 12 ft.

The measurements of the interval of uncertainty and movement error do not support the impression, gained from discussion with the subjects, of increased difficulty in making

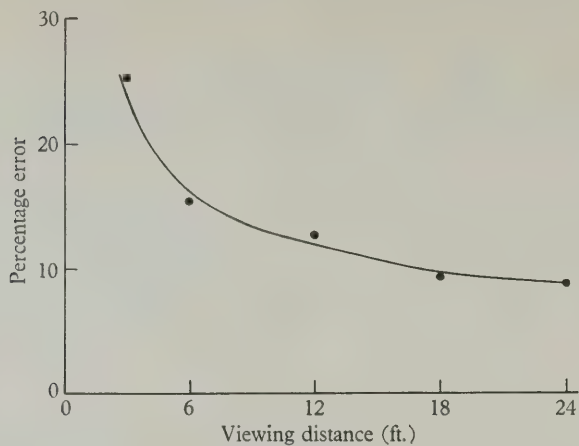
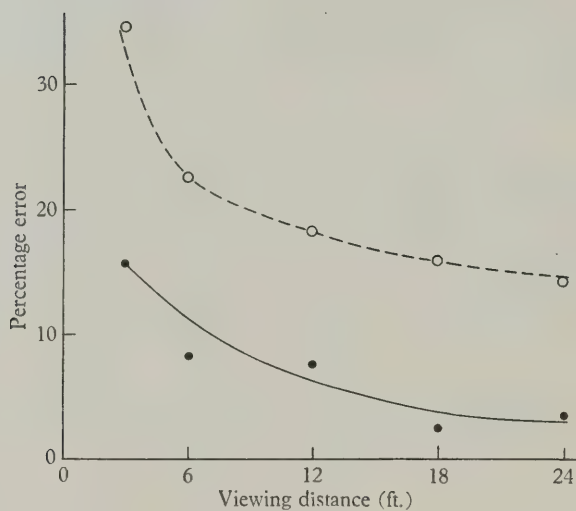
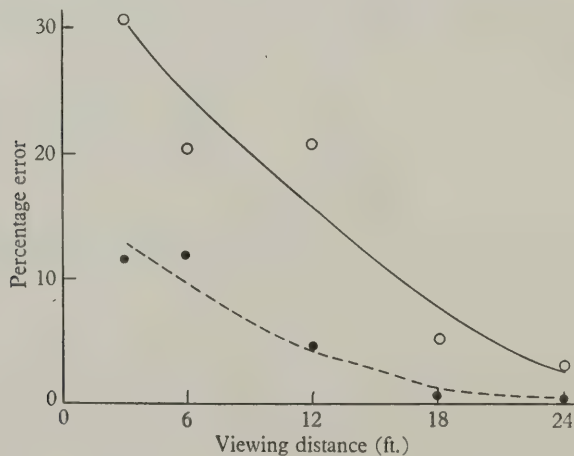


Fig. 1. Variation of depth effect with viewing distance.

Fig. 2. Relation of depth effect to viewing distance for normal and narrow angle lenses.
●—●, 'normal' lens; ○---○, narrow angle lens.Fig. 3. Relation of depth effect at variable distance to monocular and binocular regard.
○—○, binocular; ●---●, monocular.

a judgement when seated close to the screen. The relative size of the errors at different viewing distances and between the series photographed with the two different lenses is probably reduced in the experiments, owing to recognition of individual frames, mentioned above, and 'central tendency effects', as the subjects had no standard against which to make a comparison other than their knowledge of the shape of a cube.

The error of estimation is considerably reduced with monocular vision. The figures given are for two subjects who were tested at each distance under conditions of monocular and binocular vision. A number of other subjects have been tested at one distance with monocular vision during the test with binocular vision and in all cases this reduction has been found. The effect is quite apparent if one eye is closed and opened whilst viewing a suitable type of picture.

Only a single ascending and descending series of readings was taken with each subject (i.e. four sets of judgements) at each distance, owing to liability of fatigue in the subject, but it was interesting to note that, though there are individual variations, the general pattern for each individual is very similar to the combined figures of the ten subjects (see Figs. 1 and 2). The method employed may be used to measure the effect of change of other variables such as those previously mentioned, e.g. contrast, depth of focus.

The judgement has been stated to be in terms of an absolute standard, i.e. the form of a well-known and easily recognized figure such as a cube; but it may be regarded as a judgement of equality or inequality between lengths (i.e. the edges of a cube as depicted in a photograph).

The impression gained during the conduct of the experiment was that an absolute standard of judgement was used. Though the subjects could give a relatively rapid decision as to equality and inequality, difficulty was often experienced in stating the direction of the inequality, i.e. + or -.

So far as the class-room is concerned depth is not the only factor in recognition. Careful examination of detail, association of related objects, previous experience of the objects themselves and of picture interpretation are other powerful factors, but for the quick glance and the quick flood of recognition, perception of depth is of fundamental importance. Hence ideally viewing, subjects should be seated at distances greater than 12 ft. from the screen. This also applies to television, and to obtain the optimum angle of vision in conjunction with a suitable viewing distance, screen sizes would need to be much larger than they are at present.

We wish to thank Prof. R. A. C. Oliver and Prof. T. H. Pear for their advice and criticism in the preparation of this paper.

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(*Manuscript received 7 September 1952*)

INVESTIGATIONS ON DEPTH PERCEPTION IN INDIRECT VISION

BY J. L. ZAJĄC

The Psychological Laboratory, University of Edinburgh

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PART I. RELATION BETWEEN DEPTH PERCEPTION AND CHANGES OF FIXATION POINT FOR INDIRECT MONOCULAR AND BINOCULAR VISION WITH DOUBLE IMAGES

I. INTRODUCTION

The present investigation is a continuation and development of experiments on visual space perception initiated by Prof. W. Heinrich and carried out in the Laboratory of Experimental Psychology in Cracow University, Poland.

The investigation of depth perception in monocular vision was the subject of papers by Heinrich (7), Kurtz (11), Loria (12), Grzybowski (5), and Zajac (13). The general results can be formulated as follows:

If we look with one eye through a tube (in order to obtain conditions of steady fixation) at a fixation point placed against a uniform background, the position of this point in the third dimension is indeterminate. But in relation to this point we can find a surface at which points on which present no difference of distance in relation to the fixation point. This surface is called the surface of reference. (Fig. 1 represents an example of a horizontal section of this surface.) Every point placed within this surface is seen as nearer than the fixation point, and every point situated outside its surface is seen as farther away.

These facts show that monocular vision is three-dimensional, and provide a new starting point for further analysis. These findings are independent of all psychological considerations.

Further investigation by the author (14) led to the conclusion that, in binocular vision, depth perception with double images is based on the same principles as in monocular vision. This is illustrated in Fig. 2, where the curves of reference (L , R) for both eyes are shown intersecting at the fixation point. We see that the point A , situated outside both curves, is seen in double images appearing farther away than the fixation point. Point B , situated on L and outside R , is seen in double images, of which one is seen equidistant with and the other farther away than the fixation point. Point C , situated on R and inside L , is seen in double images, of which one is equidistant with and the other is seen nearer than the fixation point. Point D between R and L is seen in double images, of which one is seen nearer and the other farther away than the fixation point. Finally, the point E , situated geometrically nearer than both curves, appears in double images seen nearer than the fixation point.

Previous theories could not explain the depth perception with double images because they could not relate these phenomena to monocular vision.

In all these investigations by Prof. Heinrich and his collaborators the subjects were always asked to state what was the relative distance of an object with respect to another object which was fixated, and in this way the curves and surfaces of reference were determined. Estimation of the distance of the fixation point itself and the factors deter-



Fig. 1

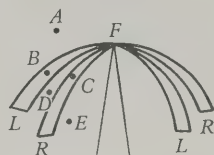


Fig. 2

Fig. 1. Curve of reference for monocular vision. Points *A* and *B* appear as equidistant with the fixation point. Point *C* is seen nearer and *D* farther than the fixation point.

Fig. 2. Curves of reference for binocular vision.

mining this estimation were not the object of these investigations. It can be said that an isolated point in the visual field is seen somewhere in space, but its distance from the eyes cannot be determined. All the geometrical constructs (e.g. the intersection of the visual axes) are theoretical arguments only, and, as such, cannot be the object of immediate perception. Further, analysis on physiological or psychological grounds, in terms of effort of accommodation on convergence, oscillation of attention, etc., seems to prove that the location in depth of fixated objects in monocular as well as in binocular vision is a problem of estimation rather than of seeing in depth. By contrast, when we have a fixation point or object we can determine the relative distance of points situated laterally: we see this

relative distance. The results of our experiments have determined the character of spatial orientation in monocular as well as binocular vision with the double images. The respective curves and surfaces of reference are not seen at a determined distance. They only represent the geometrical loci with respect to which spatial orientation in the third dimension exists. These surfaces represent the relation between the geometrical constructs and the depth perception phenomena.

Let us now pass to the physiological side of the problem. If observations are made within the range of accommodation of the eyes, sharp images of the fixation points are formed on the retinas; this is a result of the fact that the focal line, which in this case is reduced to a single point, coincides with the fovea. The images of the points situated laterally are astigmatic images which become circles or ellipses of greater or less dispersion depending on the angle between the line of vision and the visual axis and on the geometrical distance between the lateral point and the fixation object. In the case of astigmatic images two focal lines are formed. The position of the first focal line is, according to our hypothesis, the decisive factor for depth perception. When it coincides with the retina, as happens when the corresponding point is situated on the surface of reference for monocular vision, we do not see any difference in distance between the lateral object and the fixation point. We can conclude that accommodation plays its part in depth perception because it determines the situation of the focal lines with respect to the retina. Our view of the relation of the accommodation of the eyes to depth perception is thus quite different from that of writers who have supposed that the sensations and muscular efforts of accommodation are the constructive elements of depth perception.

The starting-point of this part of the investigation was the following observation made by the author and described in his paper '*La localisation en profondeur des images doubles*' (14):

If we place ourselves in a room with our back to the window and facing the opposite wall, and fixate monocularly a point on this wall, we perceive a certain spatial disposition of different objects in the room and on the wall itself. This wall has dimensions and is seen at some distance from us. If now we fixate a point situated at a much smaller geometrical distance from the eye we observe the following: the dimensions of the opposite wall are seen much smaller in comparison with those seen before, and we see this wall at a greater distance; we have a sensation as if the whole room had become longer and narrower at the same time. If we move the fixation point from near the eye towards the opposite wall, continuously and slowly enough to allow the eye steadily to accommodate on it, we observe a steady diminution of the distance of the wall and an increase of its dimensions. When we displace the fixation point in the opposite direction we perceive the opposite phenomenon, i.e. an increase of distance and a diminution of the dimensions of the wall and of other objects situated behind the fixation point.

The experiments described in the present paper were made with the object of verifying these data and exploring what happens in the case of double images formed with binocular vision.

II. EXPERIMENTAL PROCEDURE AND RESULTS

The present experiments were carried out in a long narrow room 41 ft. by 7 ft. There was a window at the end of the room and a glazed door at the opposite end. This door was illuminated by a side light. The subjects were placed near the window and looked in the direction of the door.

Two series of experiments were carried out. In the first series the position of the fixation point (a small ball 6 mm. in diameter, supported by a rod) was varied from about the near point of accommodation of the eye (eyes) to a point at arm's length, while the subject

reported on the changes in distance and size of the rest of the room, and especially of the opposite door. Observations were made with each eye separately and then with both eyes. Thirteen persons took part in this first series of experiments. In the second series the fixation point was moved steadily from near the eye (eyes) towards the door and back. Eight persons took part in this series of experiments.

All these experiments confirmed fully the phenomena related above in the case of monocular vision. The results of observations with double (binocular) images were quite similar to those with monocular vision. Among other things, this confirmed the hypothesis postulated in the writer's previous paper⁽¹⁴⁾, i.e. that the double images in binocular vision are seen in three dimensions in the same way as the corresponding single images in monocular vision.

III. DISCUSSION OF THE RESULTS

It is important to stress here the fact that the changes described above were actually *seen* by the subjects and consequently they have the value of *immediate primary data*. They were not observed as results of some kind of inference from other data. Neither were they estimates of size or distance. They had a character of something real and not of an 'illusion'. Most of the subjects spontaneously confirmed this impression and thought these experiences striking and unexpected. Only one subject expressed some doubt as to whether his observations were inferred or not, although he also confirmed the fact that he had actually seen the changes concerned. Not a single subject said that the changes observed could be a result of a comparison of the size and the distance of the fixation point with the other object seen indirectly. Several subjects noted spontaneously the continuity of the changes in the third dimension as opposed to momentary changes occurring, for example, in the case of 'depth illusions'. They found no fundamental difference between what they observed monocularly and what they saw binocularly with double images.

Helmholtz⁽⁸⁾, in a similar observation, noticed only the changes in the size of objects viewed indirectly, and he attributed these changes to the relation between the size of the finger used as a fixation object and the perceived size of the houses situated far behind. For him, the finger was the constant standard of comparison, because its linear dimensions and distance were continuously distinct in the perception, whereas this was not the case with the distant house. But we must note that this phenomenon is also observed when we do not fixate any material object, but change the accommodation of the eye without any material fixation point, which is possible after some practice. This disproves Helmholtz's interpretation of size 'illusions', as in this case there is no object which can serve as a measure.

Hering⁽⁹⁾, on the other hand, observed the changes of size or the 'illusions' of depth according to the object to which the attention was directed.

Neither Helmholtz's nor Hering's observations were complete, since both these phenomena, i.e. the changes of size and of distance, occur simultaneously.

IV. SUMMARY

All the changes observed here occurred when changes of the fixation point were made, i.e. when changes in the accommodation of the eye (eyes) took place. *The correlation between the accommodation of the eye (eyes) and the visual depth perception was thus again confirmed.*

The changes occurring when double images are formed by binocularly fixating a point are almost the same as in the case of the monocular vision. It may therefore be accepted that this phenomenon occurs according to a second principle, that *visual depth perception with double images in the binocular vision is essentially the same as in the case of monocular vision.*

The results obtained are not related to the differences which exist between space perception in monocular and in binocular vision of objects when they are fixated, i.e. in direct vision.

The investigation here described has so far given qualitative results only.

PART II. INFLUENCE OF COLOUR ON DEPTH PERCEPTION

I. INTRODUCTION

The main result obtained by Prof. Heinrich and his collaborators in their investigation on visual depth perception was that vision in the third dimension was principally correlated with accommodation of the eyes. In view of this and of the fact that accommodation must depend on the refraction of the light rays in the eye, the author put forward the hypothesis that vision in the third dimension should be different for different colours.

Dr M. Boniecka⁽¹⁾ carried out experiments based on this hypothesis, from the results of which it was possible to trace curves for different colours, representing the loci of points where objects were seen as equidistant with the fixation point. Her main results were that with a black fixation point the curve for red was situated nearer to the eye than the corresponding curve for black, while the curves for green and blue were situated farther away. These results were rather unexpected, since, on the basis of the Matthiessen formula discussed in Dr M. Boniecka's paper, one would have expected exactly the opposite results.

II. THE PURPOSE OF THE PRESENT INVESTIGATION AND EXPERIMENTAL PROCEDURE

The present investigation was conducted with three ends in view: (a) the determination of near points of accommodation of the eyes in monocular vision, using different colour filters; (b) the determination in monocular vision of the points seen at the same distance from the eye as a fixation point, viewed through a 'Q' achromatic filter, while another rod placed laterally was viewed indirectly through filters of different colours. The different colours were red, yellow, green, blue and violet. (c) Observations on differences in depth perception of double images seen through different colour filters.

The following technique was used for all these experiments.

The visual field was restricted by a white screen. The subjects looked through a rectangular slit 8.2 by 1.2 cm. The colour filters were placed over the slit. Grey metal rods of 2.75 mm. in diameter were used as the observed objects. The field of view was illuminated partly by daylight coming through the window behind the observer, and partly by a 100 W. electric bulb placed 120 cm. above the slit. This was done to ensure uniformity of illumination, and the whole resulted in a yellowish background.

In the experiments on near points of accommodation the subject looked through the slit monocularly and had first to determine the near point of accommodation without

any filters. The nearest point at which the rod was seen quite distinctly (not blurred), and without any shadows or parallel lines, was accepted as a near point of accommodation. This was determined by moving the rod towards the eye and away from it, noting the points at which the rod began to appear blurred and those at which it was again seen quite distinctly. This was repeated several times. Near points of accommodation using different colour filters were determined in the same way.

The experiment on monocular vision was conducted as follows. The fixation rod was placed in the median visual plane; while the subject fixated this rod, another rod placed laterally was moved until it appeared to be at the same distance from the eye as the fixation rod. (This rod was placed some 4 to 5 cm. of lateral distance from the fixation rod. The lateral distances in Figs. 5 and 6 are not to scale, and the differences for different colours have been given only for clearness of presentation.) The fixation rod was viewed through a 'Q' filter and the lateral one simultaneously through a colour filter.

For experiments on double images the following procedure was adopted. First, preliminary adjustments were made without using any colour filters. Two rods were placed one behind the other in the median visual plane. The nearer rod was fixated and the farther one was seen as a double image. The farther rod was displaced laterally until its two images were seen as equidistant laterally from the fixation rod. Keeping the farther rod in the same position, the subject was asked whether he observed any difference in the distance from the eyes of the two images. Both these observations were very important because the interpretation of the results was based on them, and for this reason they were checked several times during the experiments. Care was also taken that the position of the subject's head should remain unchanged throughout.

The distance from the eyes of the nearer rod was about 20 cm. and that of the farther rod 30 to 40 cm. and both were placed within the ranges of accommodation of both eyes for white light, i.e. both were seen quite distinctly when fixated binocularly or monocularly.

When in this way the positions of both rods had been determined, different colour filters were placed in front of each eye and the subjects reported which of the double images (left or right) was seen as nearer to the eyes. They were also asked to report which of them was seen as nearer to the centre (i.e. the fixation rod), which was seen more distinctly, and which appeared to be thinner. All the above-mentioned differences occurred, but there were also cases in which no such differences were observed.

When the nearer rod was fixated, uncrossed double images were obtained (the right one belonging to the right eye, and the left one to the left eye); when the farther rod was fixated the nearer rod was seen in crossed double images (the left one belonging to the right eye, and the right one to the left eye).

Two series of experiments were carried out on depth perception of double images by fixating the nearer rod and using different colour filters. The first series consisted of using a 'Q' filter (white light) in front of one eye, and in front of the other eye one of the following colour filters: red, yellow, green, blue and violet. The second series consisted of using different colours in front of each eye. In the first series there were ten and in the second twenty colour combinations.

In all the experiments only those perceptions were taken into account which took place in a short time as first impressions, since prolonged viewing was generally accompanied by inversions, image disappearances and other phenomena resulting from fatigue, ocular rivalry, colour mixture, etc.

Different kinds of Ilford colour filters were used; the tricolour and mercury type proved best suited to the majority of the observers. The corresponding wave-lengths of the filters used are shown in Table 1.

Table 1. *Wave-lengths of light passed through the filters (in Ångström units)*

Colour	Type of filter		
	Tricolour	Mercury	Spectrum
Red	5800-7200	—	6260-7200
Yellow	—	5550-7200	5580-6040
Green	4850-5900	—	5040-5440
Blue	4350-5100	—	4580-4610
Violet	—	4300-4900	4000-4610

All the experiments were carried out within the range of accommodation of the eyes of persons used as subjects. All persons tested were University students between 20 and 35 years of age whose sight, corrected where necessary by glasses, was normal. Only one person was uncertain and inconsistent in his statements. With the exceptions given below subjects had no significant differences between their two eyes.

III. RESULTS OBTAINED AND DISCUSSION

The results of the experiments on near points of accommodation are shown in Figs. 3 and 4. Two persons were tested thoroughly; a few others were tested less systematically.

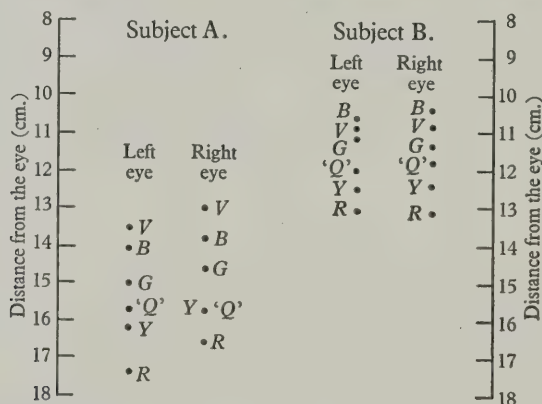


Fig. 3. Near point of accommodation (means). V, violet filter; B, blue filter; G, green filter; 'Q', 'Q' filter; Y, yellow filter; R, red filter.

The results may be summarized as follows. In general, the position of the near point of accommodation was found to be nearest to the eyes for a violet filter and farthest away for a red filter. The sequence of the distances of the near points of accommodation for different colours was found to be as follows: violet, blue, green, yellow and red.

Some discrepancies from these general results occurred for the blue and violet colour where the sequence was reversed, as shown in Fig. 3, but these might be ascribed to the fact that the difference in the wave-length of light passed through the mercury violet and the tricolour blue filters is small (see Table 1).

In the cases of young persons with normal vision the differences between the two eyes were in general not significant, as can be seen from Fig. 3, but for older people, including

the author, these differences might be considerable. In this latter case the far point of accommodation could be determined as well as the range of accommodation. This was found to be different for the two eyes and also for different colours, as shown in Fig. 4.

The results of the investigations on monocular vision are shown in Figs. 5 and 6. In this case, also, two persons were tested thoroughly and about four to seven series of observations were made for each colour and each of the fixation rods. Figs. 5 and 6 indicate that the regions of equidistance with the fixation point from the eyes are grouped in the same sequence as the near points of accommodation mentioned above, i.e. the

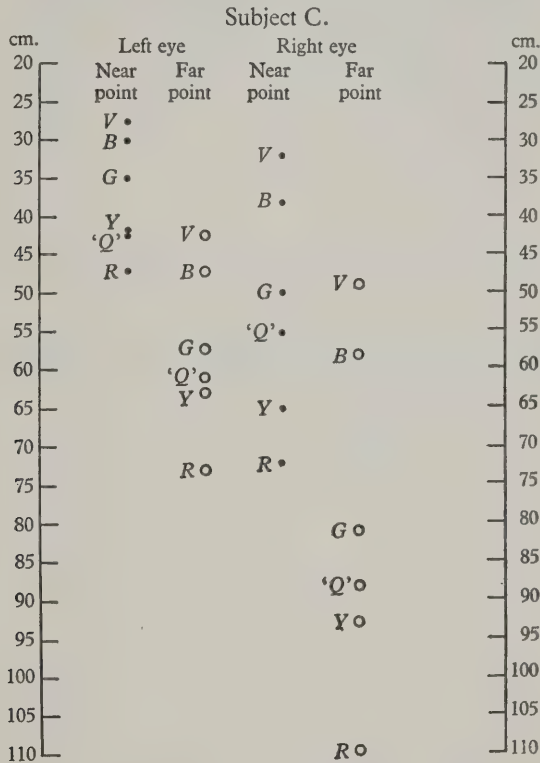


Fig. 4. Near and far points of accommodation.

equidistant region for the violet colour is placed nearest to the eye, and the colours again fall in the following order: violet, blue, green, yellow and red. They frequently overlap, especially for neighbouring colours, but it was always a striking experience to notice that if the lateral rod was placed in the equidistant position for one colour and the filter was then replaced by another colour a change in the distance of the lateral rod was generally observed. If the light passed through the filter was thus changed to a shorter wave-length the lateral rod was seen as farther away, and if the wave-length became greater the opposite was observed.

The results obtained here differ from those of Dr Boniecka's experiments, which were carried out with a different experimental arrangement, but they are in agreement with the Matthiessen formula mentioned above.

The results obtained in this series of experiments may be summarized as follows. If we fixate monocularly one rod through a 'Q' filter, and view a laterally placed rod

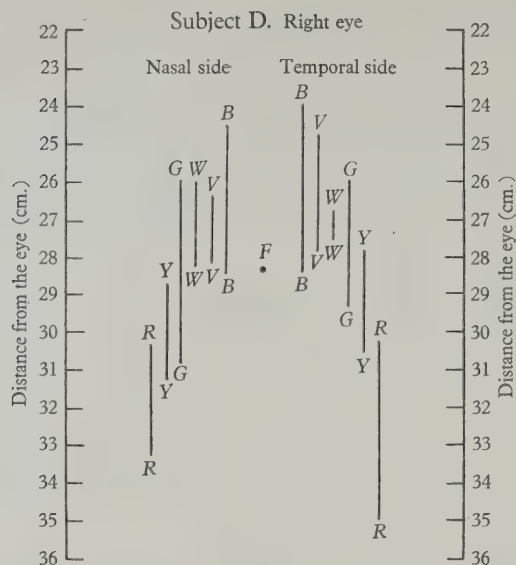


Fig. 5. Regions of equidistant positions of a laterally placed rod viewed through colour filters. Fixation rod viewed through 'Q' filter. *F*, fixation rod; *W*, without colour filter; *V*, violet; *B*, blue; *G*, green; *Y*, yellow; *R*, red.

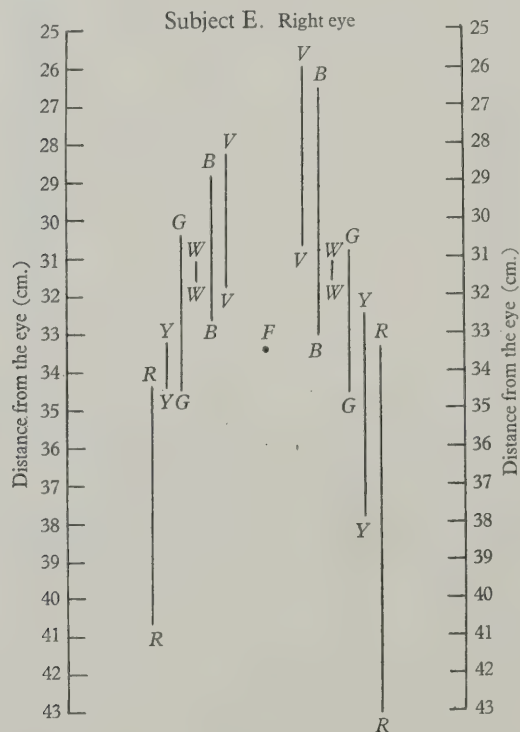


Fig. 6.

through a colour filter, and try to establish the position of equidistance by moving the lateral rod only, then we find that *the greater the refraction in the eye, i.e. the shorter the wave-length of the light passing through the colour filter, the smaller will be the geometrical distance of the lateral rod from the eye.*

The establishment of the degree of this correlation requires a more detailed investigation.

Table 2. *Binocular vision. Depth perception of double images. One eye with colour filter; other eye with 'Q' filter*

The figures represent the numbers of observations.

Colour	Image through colour filter seen			Total number of observations
	Nearer	Equidistant	Farther	
Red	29	4	2	35
Yellow	20	11	3	34
Green	9	11	15	35
Blue	1	1	33	35
Violet	0	0	30	30

Table 3. *Binocular vision. Depth perception of double images. Colour filters in front of both eyes*

Number of combination			Left image seen					Right image seen		
	Colour, left eye	Colour, right eye	Nearer	Equi-distant	Farther	Colour, left eye	Colour, right eye	Nearer	Equi-distant	Farther
1	Red	Yellow	9	9	3	Yellow	Red	9	9	3
2	Red	Green	14	5	2	Green	Red	13	5	3
3	Red	Blue	17	1	3	Blue	Red	20	0	1
4	Red	Violet	21	0	0	Violet	Red	20	1	0
5	Yellow	Green	12	5	4	Green	Yellow	12	4	5
6	Yellow	Blue	21	0	0	Blue	Yellow	18	1	2
7	Yellow	Violet	20	0	1	Violet	Yellow	17	1	1
8	Green	Blue	14	4	3	Blue	Green	15	5	1
9	Green	Violet	21	0	0	Violet	Green	15	2	2
10	Blue	Violet	9	5	7	Violet	Blue	16	6	3

The results of the experiments on double images are shown in Tables 2-7. Six subjects were tested in this series. Table 2 indicates that in the case of binocular vision the image viewed through a red filter is seen as nearer than the other image viewed through a 'Q' filter, whereas the images viewed through the blue and violet filters are seen as farther than those viewed through the 'Q' filter. The yellow filter gives nearly the same number of cases with the images seen equidistant or farther away (14) and nearer (20) as the 'Q' filter. Here, also, the results indicate that the red image is seen as nearest and the blue and violet as farthest away, which is equivalent to the results obtained with monocular vision where the regions of equidistance were in the inverted sequence. The fact is perhaps more strikingly illustrated by the results of the second series of experiments shown in Table 3, where different colours were put in front of each eye. Seven subjects were tested in this series. The greater the difference in the wave-length, the more striking are the differences in the depth perception (Tables 4 and 5).

All the results quoted above were obtained with fixation of the nearer rod.

In addition to the differences in depth perception between the two images when using different colours, several other phenomena may be observed.

When different colour filters are placed in front of each eye various phenomena occur which have been the subject of investigations by many authors; Hering⁽¹⁰⁾ among the

first of them. When different combinations of colours are used, various processes such as colour mixture, colour rivalry, etc., may take place. But in every case there exists on binocular field of vision in which the colouring and the outlines of objects may change disappear and alternate, and two monocular fields of vision, which are in general quite different from the binocular one, and which are coloured with the colour placed in front of the corresponding eye. In the binocular field of vision we see a fixation rod and double images of the farther or nearer rod. The fixation rod is more stable than the double images which undergo various changes such as inversions, disappearances, etc.; these changes are different for different durations of fixation, for different combinations of colours and for differences between the two eyes. With different colours, in addition to variations in depth perception, we also observe differences in the distinctness of the images, in size

Table 4. *Average normalized results for different combinations grouped together, from Table 3*

Number of combinations grouped together	(1) Left image seen			(2) Right image seen		
	Nearer	Equi-distant	Farther	Nearer	Equi-distant	Farther
1 + 5 + 8 + 10*	11	6	4	12	6	3
2 + 6 + 9†	18 $\frac{2}{3}$	1 $\frac{2}{3}$	$\frac{2}{3}$	15	3 $\frac{2}{3}$	2 $\frac{2}{3}$
3 + 7‡	18 $\frac{1}{2}$	$\frac{1}{2}$	2	18 $\frac{1}{2}$	1	1
4§	21	0	0	20	1	0

* Combinations of two neighbouring colours.
† Combinations with difference of two colours.
‡ Combinations with difference of three colours.
§ Combinations with difference of four colours.

Table 5. *Means of columns 1 and 2 of Table 4*

Number of combinations grouped together	Image seen		
	Nearer	Equidistant	Farther
1 + 5 + 8 + 10	11 $\frac{1}{2}$	6	3 $\frac{1}{2}$
2 + 6 + 9	16 $\frac{2}{3}$	2 $\frac{2}{3}$	1 $\frac{1}{3}$
3 + 7	18 $\frac{1}{2}$	$\frac{3}{4}$	1 $\frac{1}{2}$
4	20 $\frac{1}{2}$	$\frac{1}{2}$	0

or thickness of the rods, and in the lateral distance of the double images from the fixation rod. All these changes are interesting because they are all related to changes of accommodation. All of them require special investigation. Our experiments were mainly concerned with changes in distinctness, and it was found that in 206 cases out of 282 the images seen as less distinct were also seen as farther away. In forty-five cases the difference in distinctness was not accompanied by the perception of difference in the third dimension or vice versa. And in thirty-one cases the less distinct images were seen as nearer. Thus it may be assumed that a relation also exists between the distinctness of the image and its depth perception. It was also observed that the most distinct were the images seen through the red filter and the least distinct those seen through the violet filter. The sequence of colours in this respect was again: red, yellow, green, blue and violet, similar to that for depth perception.

All the results described seem to indicate that a definite relation exists between the wave-length of light used and the depth perception of the images in indirect vision.

Table 6. *Observations by subject M corresponding to Table 2*

Colour filter in front of the left eye			
Colour	Left image seen		
	Nearer	Equidistant	Farther
Red	3	0	0
Yellow	3	0	0
Green	1	1	1
Blue	0	0	3
Violet	0	0	3
Total number of observations	7	1	7

Colour filter in front of the right eye			
Colour	Right image seen		
	Nearer	Equidistant	Farther
Red	0	1	2
Yellow	1	1	1
Green	1	0	2
Blue	0	0	3
Violet	0	0	3
Total number of observations	2	2	11

Table 7. *Observations by subject M corresponding to Table 3*

Number of combination	Colour on left eye	Colour on right eye	Left image seen		
			Nearer	Equidistant	Farther
1	Red	Yellow	2	1	0
2	Red	Green	3	0	0
3	Red	Blue	3	0	0
4	Red	Violet	3	0	0
5	Yellow	Green	3	0	0
6	Yellow	Blue	3	0	0
7	Yellow	Violet	3	0	0
8	Green	Blue	3	0	0
9	Green	Violet	3	0	0
10	Blue	Violet	3	0	0
Total number of observations	—	—	29	1	0

Number of combination	Colour on left eye	Colour on right eye	Right image seen		
			Nearer	Equidistant	Farther
1	Yellow	Red	0	1	2
2	Green	Red	0	1	2
3	Blue	Red	3	0	0
4	Violet	Red	3	0	0
5	Green	Yellow	0	0	3
6	Blue	Yellow	3	0	0
7	Violet	Yellow	3	0	0
8	Blue	Green	2	1	0
9	Violet	Green	3	0	0
10	Violet	Blue	0	1	2
Total number of observations	—	—	17	4	9

Similar suppositions concerning direct vision have been made by several authors working on this subject, e.g. Davson⁽³⁾ and Hartridge⁽⁶⁾, although some of them, Linthoven⁽⁴⁾ and Bourdon⁽²⁾, attributed the differences in estimates of distance for different colours to changes in the size or eccentricity of the pupil (pupils).

Some differences in depth perception due to differences between the two eyes were also observed. For example, during the preliminary adjustments for the experiments,

subject M always saw the right member of the double images farther away than the left although he saw them as laterally equidistant from the nearer fixation rod. Consequently when any colour filter was placed in front of his right eye he always saw the right image as farther away than the left image seen through the 'Q' filter. On the other hand, with the colour filters placed in front of his left eye he saw red, yellow and green images as nearer, and blue and violet as farther away. This fact is indicated in Table 6, and also in Table 7, where differences in depth perception with different colour filters in front of each eye are shown.

These observations may help in devising methods of measuring various characteristics of the eyes.

IV. SUMMARY

The experiments have given the following general results:

(a) Objects viewed in indirect vision through different colour filters are seen at different distances from the eyes.

(b) This phenomenon was observed both in the case of monocular vision, and also in binocular vision with double images.

(c) A relation seems to exist between depth perception and wave-length of light. Objects are seen as nearer with longer wave-lengths and as farther away with shorter wave-lengths of light.

The author is indebted to Prof. Drever, Dr Semeonoff and Dr Mary Collins for suggestions and help in this work, to those who acted as subjects and to Dr Barbara Zając for her assistance.

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(Manuscript received 1 February 1952)

APPARENT MOVEMENT AND EYE MOVEMENTS

By C. E. M. HANSEL

Department of Psychology, The University, Manchester

Introduction to the problem (pp. 145-147). II. *Experimental procedure used* (pp. 147-149). III. *Results* (pp. 149-151). IV. *Discussion of results* (pp. 151-153). V. *Interpretation in relation to eye movements* (pp. 153-154). VI. *Summary* (pp. 154-155). *References* (p. 155).

I. INTRODUCTION TO THE PROBLEM

With the majority of human beings past experience of the external environment is sufficiently similar for a common interpretation to be placed on a large range of stimuli. In particular circumstances this common interpretation will be determined by the activity set up in the nervous system by the stimulus. Losses occur in the chain of processes from the receptors to the central mechanisms, and the variations which exist in the physical stimuli are potentially able to impart more information than is contained in the message supplied by the patterns of nerve impulses in the nervous system. This potentiality can be shown to exist by using instruments such as a magnifying lens.

In the study of visual perception it is usual to define 'cues' in accordance with those aspects of the external stimuli to which differential sensitivity is displayed by the receptors and which may affect perception. The term 'physiological cue' will here be used to denote potential information as contained in the pattern of nerve impulses which becomes available for perception.

It is assumed that if a particular set of physiological cues has always in the past arisen in particular circumstances—i.e. in conjunction with other cues—and produced a perception *P* in a subject, and now an identical set of physiological cues be caused by some other set of circumstances not before encountered, then the response or perception will be in terms of *P*.

The thesis here put forward is that the conditions in which apparent movement effects are obtained are such that they will produce a set of physiological cues of the nature normally arising in the case of moving objects, and, having in the past experience of the individual arisen in connexion with moving objects, they now produce perception of moving objects.

In life, experience tends to be in terms of objects which move from one position to another rather than of objects which vanish and reappear at a different position. There are conditions in which a moving object is hidden during a part of its movement, but such conditions arise when there is another object in the intervening space. Early experience of moving objects probably arises in part when observing the movements of one's own body or of objects held, whose permanence is indicated by other than visual cues.

If it can be shown that two different stimuli will under certain conditions of stimulation produce equivalent sets of physiological cues, it may be deduced that there will be no difference in the information by which the two sets of cues may be differentiated and perception will be in terms of the manner in which this particular set of cues reacts with past experience.

Alternatively, accepting the statement above, it may be argued that similarity of perception implies equivalence of physiological cues and this may be used as a datum in the investigation of physiological processes, it being implied that the equivalence is brought about by losses of potential information involved in the physiological processes and that a satisfactory state of knowledge regarding the physiological processes would make the loss of potential information resulting in equivalence predictable in a given set of circumstances.

Wertheimer reported apparent movement when the period between two stimuli was approximately 0.06 sec. This interval is of interest as it is encountered in other sensory phenomena such as α -adaptation, Meta-contrast, and subjective flicker rate. It corresponds to a frequency of around 18 cyc./sec. which is that frequency below which nerve impulses appear to act as units. Experiments suggest that the maximum frequency at which the retinal neurones respond is around this figure (15–25 cyc./sec.), and that they are incapable of responding much above it, higher impulse rates being achieved by volleying (3). This time interval is also encountered in motor phenomena. It approximates to the optimum repetitive rate of movement of a single muscle and to that frequency of repetition at which a stimulus too weak to elicit a reflex will produce the optimum response.

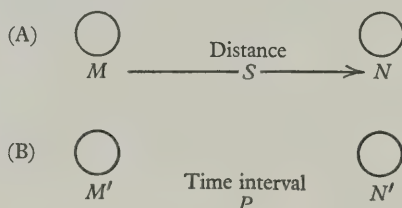


Fig. 1.

Wertheimer's interval of 0.06 sec. has not been confirmed by other investigators (4); it appears, however, to be near the minimum period for β -movement, although under certain conditions optimal movement may arise with much longer intervals.

Fig. 1A represents conditions for real movement. The stimulus moves from M through distance S to N . Fig. 1B represents conditions for apparent movement. Onset of N occurs after a time interval P from offset of M' . In the case of real movement the retinal image corresponding to the moving object moves across the retina and stimulation of the retina will occur between those positions on the retina corresponding to M and N . Under conditions of apparent movement stimulation is confined to those areas of the retina corresponding to M' and N' .

If the two sets of stimulus conditions give identical physiological cues it is implied either that the retinal neurones do not respond in real movement for positions on the retina between M and N or else that any impulses initiated in the retina are inhibited in the retina or at the brain. It would thus appear that following onset or offset of stimulus there follows a period of time in which either the photoreceptors do not respond or else any impulses produced are blocked, and that this effect spreads beyond the particular area of the retina or projection area directly involved by the stimulus. The extent of this spread may be expected to increase with stimulus intensity, and the distance over which movement effects can be obtained has been stated to increase with increase of change of stimulus intensity. Thus it may be expected, if a large field be intermittently illuminated

at a suitable frequency and a small area in the centre of the large field be intermittently illuminated at the same frequency but with different phase relationship relative to the large field, that at certain frequencies of stimulation the centre field will appear darker than the surround as its phase difference is increased towards 180° . This hypothesis has been tested and some preliminary results are given below.

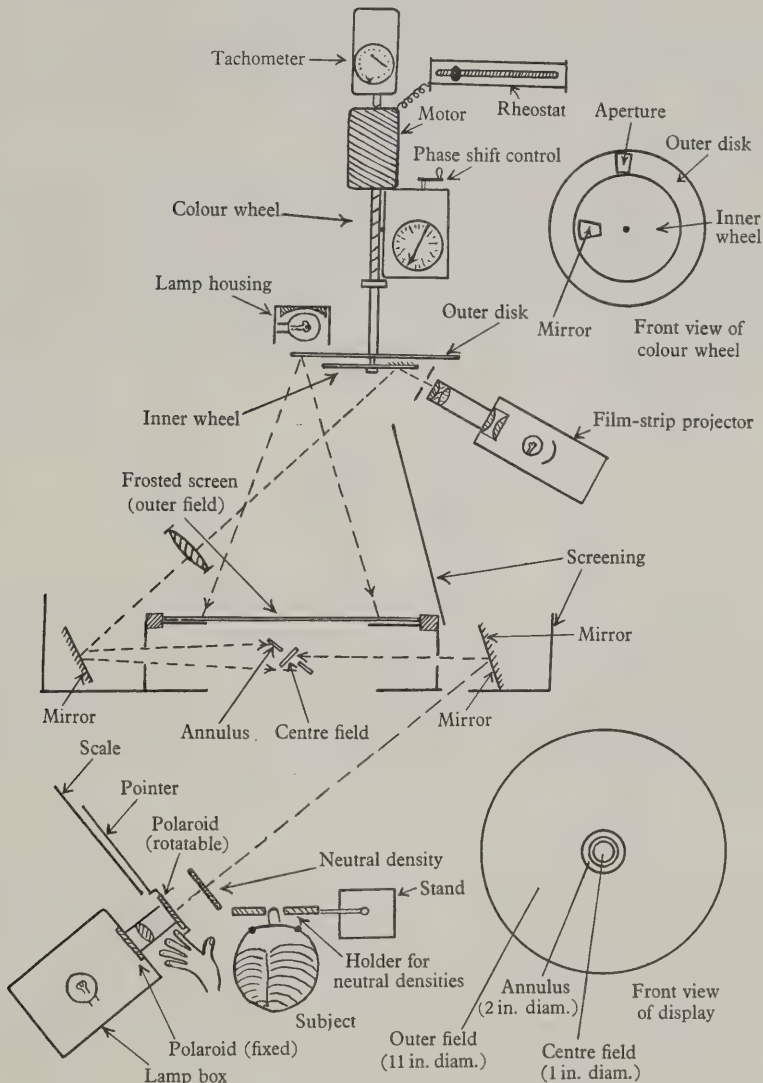


Fig. 2. Apparatus (not drawn to scale).

II. EXPERIMENTAL PROCEDURE USED

A mirror was fixed to the centre disk of a colour wheel, and a disk containing a variable aperture in the periphery was fixed on the outer wheel. The mirror was screened except for an arc of 10° . Light passed through the slit in the outer disk and illuminated from there a frosted glass screen. A beam of light was focused on to the inner wheel so that it was reflected by the mirror over 25° of its travel. This beam passed to the side and in front of the frosted glass screen and was there deflected by a fixed mirror so that it illuminated

a small white annulus (2 in. diameter) standing at an angle in front of the frosted glass screen (Fig. 2). The colour wheel was of the type with which it is possible to move the inner wheel relative to the outer through 360° whilst rotating, the angle moved through being indicated on a dial. It was thus possible to change the phase relationship of the light reflected from the mirror and that passing through the aperture on the outer disk. The annulus contained an inner circle which was illuminated by a beam from the other side of the screen. The intensity of this beam could be varied by the subject and measured by the experimenter: it was not alternating and was used to determine the subjective brightness of the annulus, adjustments to its intensity being made by the subject until the ring and the annulus appeared alike.¹

Single readings were taken at each position of phase shift as it was found that the experiments were fatiguing to the subjects and if more readings were taken in order to increase the accuracy of measurement the experiments became unduly prolonged. In order to give some idea of the consistency of the readings one subject was tested at four positions of phase shift, ten readings being taken at each position. In each set of ten readings five were taken increasing the illumination of the centre area and five decreasing it so as to obtain a measure of movement error.

Results were as follows:

Phase shift ($^\circ$)	(Frequency 6 cyc./sec.)		Movement error
	Mean Light value	Standard deviation	
0	0.895	0.160	- 0.017
90	0.157	0.026	- 0.009
180	0.365	0.031	- 0.011
270	0.623	0.091	- 0.016

In all the experiments to be described the subjects sat 5 ft. from the screen. The outer field was 11 in. in diameter, the annulus 2 in. in diameter. The apparatus was adjusted so that the outer field and annulus were equally illuminated over 25° of the cycle. This adjustment was checked with a photoelectric-cell and oscilloscope. The maximum illumination level was equivalent to 1.6 f.c. as measured with a Weston meter. Reductions in illumination level were obtained by the subject viewing through neutral density filters having transmission coefficients of 0.1, 0.01, 0.001, 0.0001.

Experiment I

The subjects were tested to find the range of time intervals separating two stimuli for which apparent movement effects were observed. The outer field was screened except for a circle the size of the annulus and 4 in. from it. The phase difference between illumination of the annulus and outer field was set at 180° . The range of frequencies was noted at which the subject reported movement as the frequency was increased slowly from zero. In this case the interval between the two stimuli was $1/2F$ where F is the frequency of rotation in cycles per second, or, allowing for the duration of the stimuli, $155/360F$ sec. and the duration of each stimulus $25/360F$ sec.

Experiment II

Adjustments of the inner circle to bring about equality between it and the annulus were made at 30° intervals of phase shift from 0 to 360° , frequency being kept constant. Readings were taken at frequencies varying from 5 to 20 cyc./sec.

¹ Since these experiments, the apparatus has been re-designed in order to reduce stray light, increase the range of light intensities and give more precise control of flicker frequency.

Experiment III

Black circular disks of varying diameters were placed between the annulus and the frosted glass screen, thus introducing an unilluminated area between the annulus and the outer field. Readings were taken with varying sizes of this unilluminated area.

Experiment IV

Readings were taken as in Exp. II but the illumination level of the annulus and outer field were varied.

III. RESULTS

Experiment I. The apparent movement effect

The optimal time interval between stimuli with the illumination 1.6 f.c. was about 50 msec. Movement was reported over a range of intervals varying from 330 to 118 msec. The interval of 330 msec. was the largest that could be produced with the apparatus. At the extreme values movement was reported as intermittent and sometimes as being jerky. The optimal movement effect was often described as resembling a pendulum.

Variations with light intensity

Values for one subject:

Light intensity (f.c.)	Optimum (msec.)	Lower limit (msec.)	Upper limit (msec.)
1.6	150	200	118
0.16	220	287	172
0.016	260	(330)	198
0.0016	270	(330)	216

The movement effect is reported over a wide range of time intervals and it is difficult to assess precise limits. The above time intervals are also not directly comparable with the other experimental data as in the conditions for observing movement the stimulus duration relative to the time interval separating the stimuli is double that of the other experiments.

Experiment II. Inhibition effect on small field

Subjects were tested at frequencies from 5 to 20 cyc./sec. Individuals were fairly consistent on retest but large individual differences were found.

Fig. 3 shows results for one subject at different frequencies.

Fig. 4 shows results for one subject taken at the same frequency but on different days.

Fig. 5 shows results for different subjects at a fixed frequency.

Some subjects had no difficulty in giving judgements. Other subjects experienced alternation effects at certain degrees of phase shift.

(a) Rapid alternation which appeared usually at the position of maximum slope in the curves of Fig. 5.

(b) Slow alternation at a period of one second or more per cycle with small amounts of phase shift.

In the rapid alternation, variation was between two extremes—bright, dark. Usually one phase predominated. Thus the optima in some of the curves in Fig. 5 may be due to these subjects matching with the bright phase, whilst with the remaining subjects the dark phase only was present or dominant. Some subjects did not report alternation effects. Other effects such as contour distortion, movement, coloration and glitter were

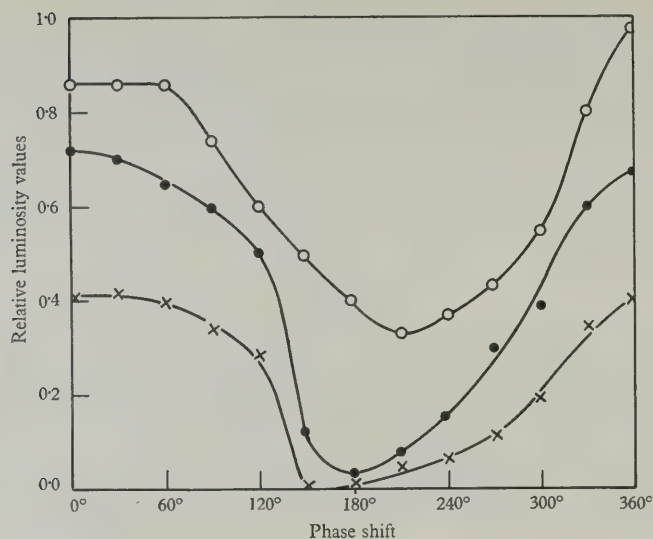


Fig. 3. Readings from one subject at three frequencies. ○, 11 cyc./sec.; ●, 8 cyc./sec.; ×, 6 cyc./sec.

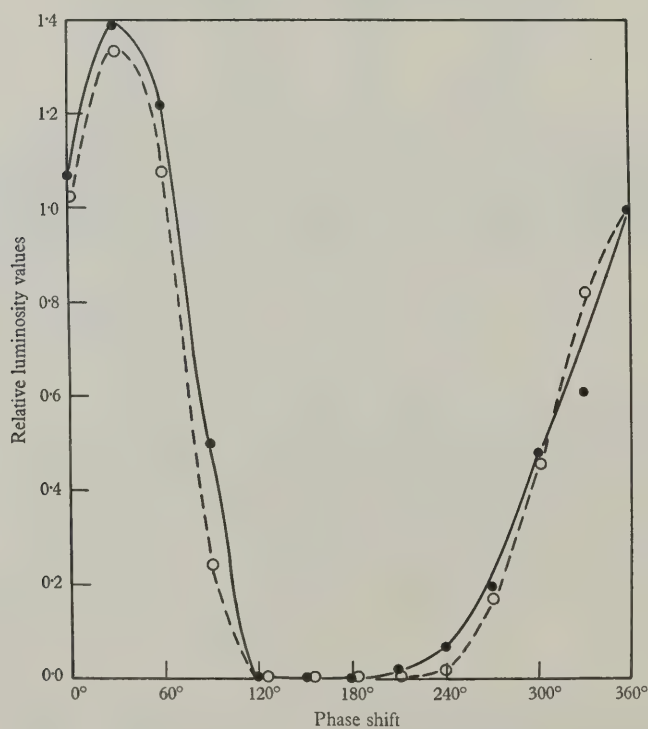


Fig. 4. Readings for one subject taken on different days.

also reported. In some cases alternation was not of the annulus but of the central evenly illuminated area: in other cases, change of illumination of the central area appeared to affect the annulus.

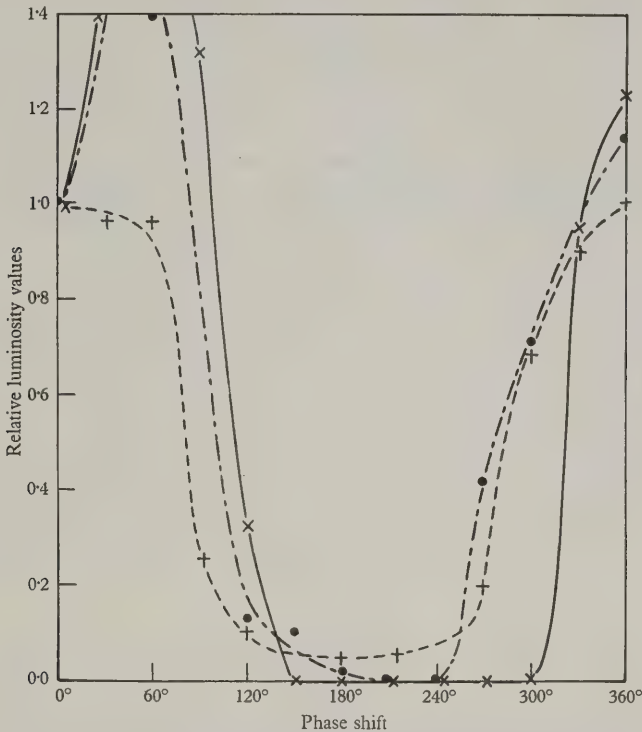


Fig. 5. Readings of different subjects at a frequency of 6 cyc./sec.

Experiment III

Fig. 6 shows the effect of interposing an unilluminated gap between the annulus and outer field. It will be seen that even a small gap changes the results obtained from an inhibiting to a facilitating effect as phase difference is increased towards 180°.

Experiment IV

Fig. 7 shows results for one subject at a frequency of 6 cyc./sec. for different levels of overall illumination. Neutral density filters were placed in front of the eyes of the subject as decreasing the illumination level of each field. It will be seen that as the general illumination is decreased the illumination of the central area has to be increased in order to give a match even where there is no phase difference between the annulus and outer field.

IV. DISCUSSION OF RESULTS

A number of the effects observed are similar to those obtained when an intermittently illuminated field is surrounded by an evenly illuminated annulus and which are discussed by Bartley (1). Under these conditions it is reported that the dark phase of the intermittent stimulus may be made to predominate by increasing the intensity of the surround at low frequency rates. In the experiments described above the outer field was of equal

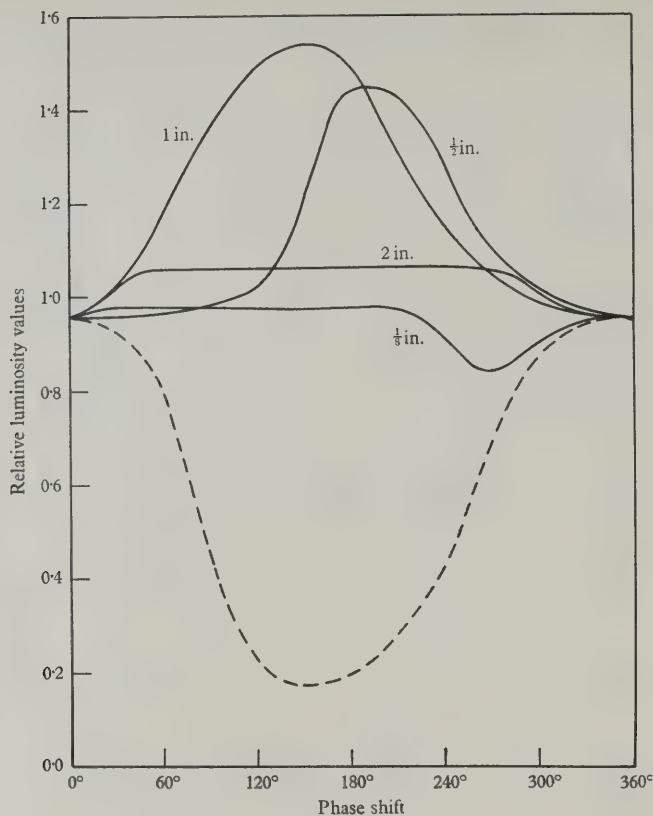


Fig. 6. Effect of unilluminated area separating the two fields. ---, fields touching; —, unilluminated area of width shown against the curve.

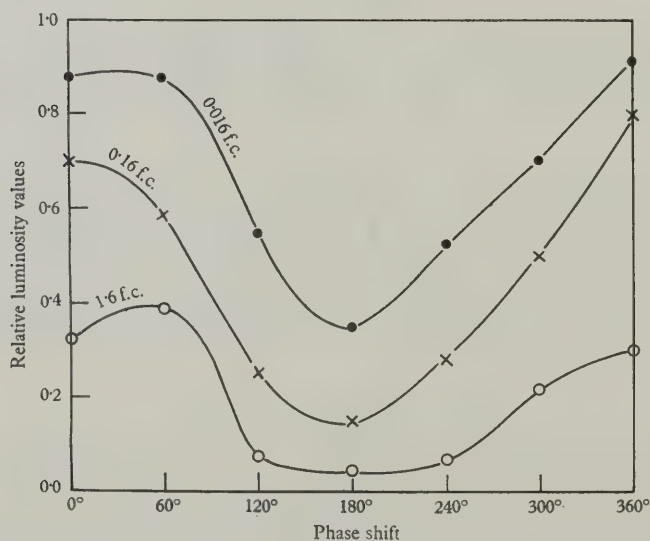


Fig. 7. Readings for one subject at three illumination levels. Frequency 6 cyc./sec.

intensity to the annulus, but was alternating out of phase with it. It should also be noted that its area was large in relation to that of the annulus.

The effect may be related to α -adaptation if it be assumed that recovery is as rapid as the adaptation effect. It would then follow that when the frequency of stimulation is less than a minimum value in which the full cycle of adaptation and recovery can occur, there will be a decrease in the relative effect at different degrees of phase shift, and a decrease in the effect with no phase shift. Bartley's results⁽¹⁾, which have been confirmed using the present apparatus, show that, with a single alternating field, optimum brightness is experienced at frequencies around 10 cyc./sec. or around the α -rhythm frequency. It is, however, found that the relative effect at different degrees of phase shift is less pronounced with some subjects as the frequency is increased above a figure of about 6 cyc./sec., and at these low frequencies there is a positive part of the cycle. Experiments are at present being made to determine the relationships involved with variation in frequency of stimulation, stimulus intensity, and area of stimulus. With two subjects so far tested in which the flash frequency is varied, the effect persists up to 12 cyc./sec. and is therefore possibly due to synchronization of the α -rhythm by the outer field.

Experiments have not yet been carried out in which the relative levels of illumination of the annulus and outer field are different or where fixation is other than on the centre of the display. Preliminary tests have been made to determine the maximum size of the smaller field which can be affected by the outer field, and these indicate a considerably larger area than that used in the experiments above.

V. INTERPRETATION IN RELATION TO EYE MOVEMENTS

It is interesting to note that whereas movement effects can be obtained from stationary stimuli there is absence of perception of movement when it might be expected during eye movements. It is stated that the effects of eye movements may be perceived by rapidly moving a mirror in which a display is being viewed⁽⁶⁾. Under such conditions, however, movement is not of the eyes but of the display, and the duration of the movement is indeterminate. In everyday life, when the eyes are rapidly scanning an object with small movements, such effects appear to be lacking. The periods of eye movements as reported by Dodge and Cline⁽²⁾ are given below:

Extent of movement (°)	Duration of movement (msec.)
5	29
10	39
15	48
20	55
30	80
40	100

Duration of eye movements (data of Dodge and Cline from Woodworth, *Experimental Psychology*).

It will be seen that movements of 20° and less occur in less than 60 msec. There is thus the possibility of their occurring during an inhibitory period, of the type investigated above, either by motor synchronization or by onset of movement initiating an inhibitory period through on-off fibres coming into play at changes of stimulus illumination. The two effects may be interrelated in that stimulus flash frequency can synchronize the α -rhythm and motor discharge causing eye movements may be dependent on phase

relationship to the α -rhythm.* A movement of 20° across the page of a book 1 ft. from the eyes covers 4 in. If an attempt is made to sweep back across a much larger distance the effects of movement become quite apparent. A distance corresponding to 20° each side of the fixation point corresponds also to the value quoted as the normal binocular motor field (6). When scanning a display, changes of fixation point may be expected to be within such a field.

Quite marked individual differences have been obtained when taking measurements at a fixed frequency. The steep curve with a large maximum at low degrees of phase shift has been obtained with most but not all subjects. Subjects also vary markedly with respect to the minimum value at between 180 and 240° phase shift. With some subjects the small amount of stray light present prohibits making accurate measurements at this position. If the effect is due to a mechanism involved in viewing moving stimuli or during eye movements, it is possible that these individual differences may be dependent on a visual function, other than visual acuity, which is of importance during vision of moving objects or when the eyes are scanning a static display. It is thus considered that the phenomenon of apparent movement, whilst not directly caused by movement of the eyes, may arise because there is a mechanism whose function in the seeing of everyday objects is to increase the clarity of vision when the eyes are in a state of movement. The conditions in which optimum apparent movement effects are obtained are not in general those of everyday life and the visual cues elicited are not differentiated by the visual apparatus from those produced by actual moving bodies. In the case of the laboratory experiment using illuminated areas the emphasis is on physiological cues in relation to past experience. With more meaningful and complex types of stimulus, particularly under conditions of reduced cues or where only a portion of a multiplicity of cues is selected and utilized by the subject, past experience may play a more preponderant role and compensate to some degree for differences which may be present in the physiological cues and those which would be present with real movement. An instance of the priority of past experience over physiological cues may be seen when viewing stereoscopic pictures with the left eye and the right eye views reversed. Under such conditions it is not possible to obtain reversal of depth effects with familiar objects such as the human face, although the binocular cues would indicate such a reversal and produce reversal with less familiar objects.

VI. SUMMARY

Apparent movement may be due to inability of the visual mechanisms to provide, under the stimulus conditions in which they are obtained, physiological cues which differ from those evoked by moving objects.

Experiments have been carried out which show that intermittent excitation of a large area of the retina at a suitable frequency may produce inhibition of response on a small area intermittently illuminated at a similar frequency but out of phase. Such an inhibition effect is capable of reducing the physiological cues produced by real movement so that they are similar to those produced under stimulus conditions for apparent movement.

* Since this was printed I have seen a paper by Dr J. A. V. Bates (*J. Physiol.* cxiii, 440) in which he describes experiments showing that when alpha activity is present there is a significant tendency for the instant of voluntary movement (in this case of flexor muscles of the forearm) to be related to the phase of the alpha rhythm. Dr Bates tells me that he has been informed of experiments in which movements of the eyes in nystagmus, induced by looking at a rotating drum, can be in phase with the alpha rhythm.

It is postulated that not all the information provided by excitation of the retina under conditions of real movement is utilized by the brain. This seeming deficiency is thought to be due to a mechanism of use in providing increased efficiency of vision by suppressing signals from the retina which are produced during eye movements.

I would like to thank Prof. L. S. Hearnshaw who has read and criticized the script, Mr R. G. Church who has assisted with the experiments and apparatus, and students and members of the Department who have acted as subjects.

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(*Manuscript received 13 August 1952*)

A PROJECTIVE TECHNIQUE BASED UPON THE KINAESTHETIC AND TACTILE MODALITIES

BY F. V. SMITH AND SILLOO K. MADAN

Department of Psychology, University of Durham

I. *Introduction* (p. 156). II. *Development of the materials and methods of the present study* (pp. 156-157). III. *The technique of presentation* (p. 157). IV. *Recording* (pp. 157-159). V. *Scoring* (p. 159). VI. *Appraisal* (pp. 159-162). VII. *Conclusion* (p. 162). *References* (p. 162).

I. INTRODUCTION

The number of influences contributing to the behaviour of a person is obviously large and varied. A full account of all the causes which have contributed to even one action by the person is often an ideal which is beyond practical attainment. Long periods of close acquaintance with the person and in a variety of situations may enable an observer to guess fairly successfully at the predominant trends of the personality and even at their aetiology; but, clearly, this degree of intimacy is not always attainable and the demand for the services of professional people requires that the time spent with subjects should reveal as much relevant information as possible.

For this reason, various aids which intensify the exploration which can be achieved in a given time have been devised. Well-known examples are the Rorschach Test⁽¹⁵⁾, the Murray Thematic Apperception Test^(12, 13), Stern's Cloud Pictures⁽¹⁶⁾, the Lowenfeld Mosaic test⁽⁷⁾, and Mira's analysis of the lines reproduced by blind-folded subjects drawing with the unsupported arm^(8, 9, 10). Other procedures include those of Angyal⁽¹⁾ in which the subjects reproduce letters presented in a particular order, the finger painting methods of Napoli & Harris⁽¹⁴⁾ for the partially sighted and Field's analysis of aimless drawings or 'doodles'⁽⁵⁾. Recently, we have learned of the use of unstructured forms in bas-relief by Harris⁽⁶⁾ and of three-dimensional forms by Twitchell-Allen⁽¹⁷⁾.

II. DEVELOPMENT OF THE MATERIALS AND METHODS OF THE PRESENT STUDY

In any process of perception, the previous experience of the subject in some way contributes to the meaning which emerges. Some early studies in the use of ink-blot as projective material by Dearborn⁽³⁾ in 1897-8 and Feingold⁽⁴⁾ in 1915 provide interesting illustrations of this thesis. The studies made by Münz & Löwenfeld⁽¹¹⁾ of the work in clay by boys who were congenitally blind or blind from an early age, demonstrated the considerable contribution which kinaesthesia, and states of feeling such as lassitude, can make in perception. The work of Cutsforth⁽²⁾ also suggested, as have repeated observations of other forms of synaesthesia, that experience in the tactile and kinaesthetic modalities can influence visual experience. Werner⁽¹⁸⁾ too, has indicated the influence of internal impressions of muscular activity on children's drawings.

The frequent use made of the sense of muscular effort and its association with so many of the bodily movements and manipulations of common objects upon which observers base their judgements of persons, suggested that objects varying in size, shape and weight in a variety of materials, could serve as stimuli from which interesting and possibly revealing projections could be made.

Accordingly, a procedure was evolved in which ten objects were placed singly under a light demountable screen which prevented the subject from seeing his hands, but enabled him to manipulate the object freely. The objects differed in size, weight, shape, surface texture and compressibility. Object 3 was presented with a wet surface and the putty of object 10 was always greasy. The general nature of the objects may be gathered from the accompanying illustration. Other details in the order: material, greatest length, weight, surface, are:

Object 1, unglazed porcelain, $4\frac{1}{2}$ in., $19\frac{1}{2}$ oz., rough. Object 2, glazed porcelain, 5 in., 30 oz., smooth. Object 3, glazed porcelain, 6 in., 22 oz., smooth. Object 4, wood, $7\frac{1}{2}$ in., 3 oz., rough. Object 5, wax, $4\frac{1}{2}$ in., 7 oz., smooth. Object 6, glazed porcelain, $6\frac{1}{2}$ in., $23\frac{1}{2}$ oz., smooth, with inserted bristles. Object 7, wood, 5 in., 3 oz., smooth. Object 8, glazed porcelain, $6\frac{1}{2}$ in., 46 oz., smooth. Object 9, rubber, $7\frac{1}{4}$ in., 62 oz., rough. Object 10, putty, 7 in., 14 oz., smooth and greasy.

The arrows enabled the interviewer to present the object to each subject in the same way. The small figures and symbols were painted on the surface so that a detailed account of the particular areas which evoked responses could be made. The nature of the ten objects, the method of presentation and the method of recording and scoring were evolved over a period of 5 months, beginning in early December 1950, employing thirty-five subjects and a variety of materials in different shapes. The criteria determining the final form of each object were the number and variety of responses evoked by it. The total weight and bulk of the objects and screen were such that the whole equipment could be carried in a small grip from one place of work to another.

III. THE TECHNIQUE OF PRESENTATION

The subject was seated comfortably at a table, opposite the interviewer. The object and the subject's hands were screened from him; but he could see everything that the interviewer did and could converse freely while handling the particular object under discussion.

The interviewer began: 'It's very good of you to come here and help me with this simple project. There is nothing difficult about it and there are no right and wrong answers. I shall place several objects one at a time under this screen and you can tell me everything that occurs to you, without actually looking at the object. Of course, I can't possibly remember everything you say, so that I shall write a few notes on this paper.' No definite time limit was set for each object; but the approximate time for each response was recorded. Another object was substituted when the subject indicated that he had nothing more to say or when, after a period of silence, he answered 'Yes' to the question, 'Would you like to try another one?'

IV. RECORDING

The prevalence of different forms of movement, such as tapping, prodding and grasping and the features of each object so treated were also noted by using the appropriate numbers and letters from the recording sheet. The position of the arrow head when a verbal response was given was marked on the ellipses arranged to represent the three planes. The hand and fingers used were also recorded.

Verbal responses were noted carefully and if, when a particular object was about to be laid aside, the interviewer was not sure whether the form of the object or the surface-

texture had prompted the response, the subject was invited to elaborate. Any additional information was recorded under 'Enquiry'. Associations with other sensory modalities, such as the observation that the object suggested a brown sea-urchin and the smell of the sea, were noted at all stages and scored in separate categories indicating olfactory and visual associations, and the association of colour. The technique of recording was repeatedly revised and practised in the preliminary phase, so that efficiency and an easy, informal manner could be maintained. The average time for the entire procedure was 30 min.

V. SCORING

The scoring of the recorded data was done as soon as possible after the interview. The scoring categories are indicated in the accompanying facsimile. The 'reactions to sensations' and the 'reactions to structure' refer to the verbal responses. If the subject quickly withdrew his hands from a greasy, wet, or prickly surface, this was noted in the section entitled 'Comments'. The category light/heavy recorded a verbal observation that the particular object was light or heavy.

VI. APPRAISAL

For use in interviewing a wide variety of persons ranging from successful citizens in a wide range of occupations, graduates and undergraduates, to patients in mental hospitals, the technique has proved entirely satisfactory. In 311 interviews arranged after the preliminary phase, there has not been one case of refusal to co-operate. The presence of an object which can be moved about at will and serve as a basis for conversation appears to be a definite advantage. If the simple observations such as 'Something hard', 'It's heavy', 'A wet thing', are pleasantly received, most subjects very soon pass on to remarks of the order, 'This bit reminds me of —' and 'This top part is rather like an ornament we had in the garden'. The interviewer not only has the information from the verbal responses; but can note the way in which the object is handled and moved and those parts which appear most significant. The time necessary for the 'inquiry' is thus considerably reduced.

Present indications are that the technique can, in the manner of other projective devices, elicit information of clinical significance. Items from the previous experience of the patient, which would not normally arise in the usual type of interview, do emerge and can be discussed. It is also of interest that the data so far assembled do reveal differences between some of the major diagnostic classifications, in respect of some of the present scoring categories. For this purpose, 'Normal' is defined as a person successfully meeting his occupational and social obligations. The abnormal classifications were those recorded on the case sheets at three mental hospitals. The ideal situation would be that in which the patient was classified by a panel of diagnosticians and became available for interview before any treatments were applied. It has proved difficult to approach this ideal with a large number of cases. To date, the results of 40 interviews with normal persons, average age = 24.125, $\sigma = 4.2$, can be compared with those of 40 cases recorded as simple schizophrenia, average age = 30.1, $\sigma = 6.406$, and with 40 melancholics, average age = 59.125, $\sigma = 9.419$. The difference in average age for the normals and the schizophrenics was not significant. Within the normal group, the total number of verbal responses for those over 25 years of age was compared with that for those under 25 years

and gave an insignificant 't'. The same comparison for the schizophrenics above and below 30 years of age also gave an insignificant 't'. It would have been desirable to match these groups in age with greater precision; but we were naturally obliged to work with persons admitted to the hospitals on the grounds of illness and not for considerations of statistical nicety. Of the 20 males in the schizophrenic group, 10 had not received any shock treatments and 10 had received six or more treatments by E.C.T. The 20 female schizophrenics were similarly divided and so too, the 20 males and females of the melancholic group. The remainder of the 311 cases interviewed could not be included in this comparison because of some disqualification arising from discrepancies of age, treatments, or another classification in which, to date, the numbers are too few.

The difficulty in making statistical comparisons between the different groups is further augmented by the observation that in several of the scoring categories very few entries occur. In others, the distribution follows no clear pattern. In this preliminary study, we have no information of the ultimate nature of the distribution in each of the categories and, for the moment, we are unable to secure sufficiently large numbers of the appropriate clinical classifications to give a clear indication of the nature of the distributions. This is a severe limitation and it is possible that the recognized techniques and the usual safeguards may not meet the particular situation here encountered. The results are:

(1) *Inter-correlations between major scoring categories*

Group	No. of verbal responses and reactions to sensations	No. of verbal responses and reactions to structure	No. of verbal responses and no. of movements
Normals	$r = 0.608$ sig. at 0.01 level	$r = 0.250$ not sig.	$r = 0.779$ sig. at 0.01 level
Schizophrenics	$r = 0.006$ not sig.	$r = -0.034$ not sig.	$r = 0.565$ sig. at 0.01 level
Melancholics	$r = -0.106$ not sig.	$r = 0.019$ not sig.	$r = 0.088$ not sig.

(2) *Comparison of the responses of normal and schizophrenic groups*

	Normal mean	Schizophrenic mean	<i>t</i>	Sig. at
No. of verbal responses	15.375	10.850	2.433	0.02 level
Recognitions of materials ¹	5.625	3.275	4.989	0.01 level
No. of movements	36.050	26.025	3.504	0.01 level

(3) *Comparison of the responses of normal and melancholic groups*

	Normal mean	Melancholic mean	<i>t</i>	Sig. at
No. of verbal responses	15.375	6.1	5.472	0.01 level
Recognitions of materials	5.625	3.275	4.989	0.01 level
No. of movements	36.05	19.175	6.421	0.01 level

(4) *Comparison of the responses of schizophrenic and melancholic groups*

	Schizophrenic mean	Melancholic mean	<i>t</i>	Sig. at
No. of verbal responses	10.850	6.100	4.402	0.01 level
Recognitions of materials	3.275	3.275	—	—
No. of movements	26.025	19.175	2.762	0.01 level

For each of the above comparisons, (2), (3) or (4), a corrected χ^2 gave a consistent significance.

¹ 'Recognitions of materials'—when the subject voluntarily named the material of which the object was made.

(5) The distribution of the total number of responses for the three groups, normal, schizophrenic and melancholic, for the eight categories listed under 'content' were tested against independence values by the χ^2 technique.

χ^2 was 68.603 for D.F. = 14, $P < 0.01$.

Similarly, the total number of responses for the three groups in the nineteen categories under the heading of 'Movements' gave

$\chi^2 = 253.873$, D.F. = 36, $P < 0.01$.

These results should be regarded with caution since it was found on inspection that large scores by a few persons appreciably raised the totals for some of the categories.

(6) In order to offset the influence of the total number of responses upon entries in particular categories, such as whole (*W*) and detailed (*D*), the number of responses in each category made by individual subjects was expressed as a percentage of his total responses. The schizophrenic and melancholic groups gave more *W* responses than the normal group. The normal group, however, gave more *D* responses than the schizophrenic and melancholic groups. Comparisons by means of χ^2 corrected for D.F. = 1, were:

	Normal-schizophrenic	Normal-melancholic	Schizophrenic-melancholic
<i>W</i>	$\chi^2 = 12.94$, $P < 0.01$	$\chi^2 = 4.822$, $P < 0.05$	$\chi^2 = 0.227$, $P < 0.62$
<i>D</i>	$\chi^2 = 12.564$, $P < 0.01$	$\chi^2 = 9.917$, $P < 0.01$	$\chi^2 = 0.028$, $P < 0.90$

The distribution of responses in the other categories did not appear to justify the use of 't' technique.

(7) A comparison on the basis of 'score/no score' within each of the scoring categories listed under 'Reactions to sensations' and 'Reactions to structure' by means of χ^2 , corrected for D.F. = 1, gave the following results for the comparisons normal-schizophrenic and normal-melancholic respectively:

(i) *Reactions to sensations*

Smooth, $\chi^2 = 9.40$, $P < 0.01$; $\chi^2 = 12.0$, $P < 0.01$. Rough, $\chi^2 = 45.7$, $P < 0.01$; $\chi^2 = 66.7$, $P < 0.01$. Hard, $\chi^2 = 16.5$, $P < 0.01$; $\chi^2 = 25.1$, $P < 0.01$. Cold, $\chi^2 = 11.3$, $P < 0.01$; $\chi^2 = 31.6$, $P < 0.01$. Greasy, $\chi^2 = 14.2$, $P < 0.01$; $\chi^2 = 30.6$, $P < 0.01$. Tacky, $\chi^2 = 5.2$, $P < 0.05$; $\chi^2 = 32.5$, $P < 0.01$.

(ii) *Reactions to structure*

Holes, $\chi^2 = 49.3$, $P < 0.01$; $\chi^2 = 38.9$, $P < 0.01$. Bristles, $\chi^2 = 12.0$, $P < 0.01$; $\chi^2 = 27.1$, $P < 0.01$. Flat, $\chi^2 = 5.6$, $P < 0.02$; $\chi^2 = 21.6$, $P < 0.01$. Round, $\chi^2 = 9.1$, $P < 0.01$; $\chi^2 = 22.8$, $P < 0.01$. Curved, $\chi^2 = 7.4$, $P < 0.01$; $\chi^2 = 9.4$, $P < 0.01$.

(iii) *Weight*

	Normal-schizophrenic	Normal-melancholic	Melancholic-schizophrenic
Light	$\chi^2 = 12.6$, $P < 0.01$	$\chi^2 = 1.23$, $P < 0.30$	$\chi^2 = 4.8$, $P < 0.05$
Heavy	$\chi^2 = 34.9$, $P < 0.01$	$\chi^2 = 11.02$, $P < 0.01$	$\chi^2 = 4.8$, $P < 0.05$

(iv) *Olfactory associations*

The olfactory associations for the normal-schizophrenic comparison gave

$\chi^2 = 65.7$, $P < 0.01$.

(8) Of the male schizophrenics, 10 males had received electro-convulsive therapy and the other 10 had not. The 't' for the difference in number of verbal responses was 2.47, significant at the 0.05 level. The means were 15.2, $\sigma = 6.7$, and 8.4, $\sigma = 4.8$, respectively.

(9) Ten male melancholics receiving E.C.T. made more movements ($t=2.723$; sig. at 0.02 level) than the 10 not receiving treatment. The means were 20.7, $\sigma=1.9$, and 16.8, $\sigma=3.8$, respectively. The corresponding difference for female melancholics was $t=2.544$, sig. at 0.05 level. The means were 23.5, $\sigma=7.96$, and 15.7, $\sigma=4.55$, respectively.

(10) A comparison of an 'academic' group consisting of 10 males (university staff, post-graduate and senior undergraduates) with a non-academic, 'clerical' group, matched for age, gave an insignificant 't', for the total number of verbal responses. However, a similar comparison of the 'academic' male group with a male 'artisan' group, gave a 't' of 2.95 (sig. at 0.05 level). The total number of verbal responses given by 10 'academic' women, when compared with those of 10 'non-academic' women (shop assistants and housewives) gave a 't' of 2.54 (sig. at 0.05 level).

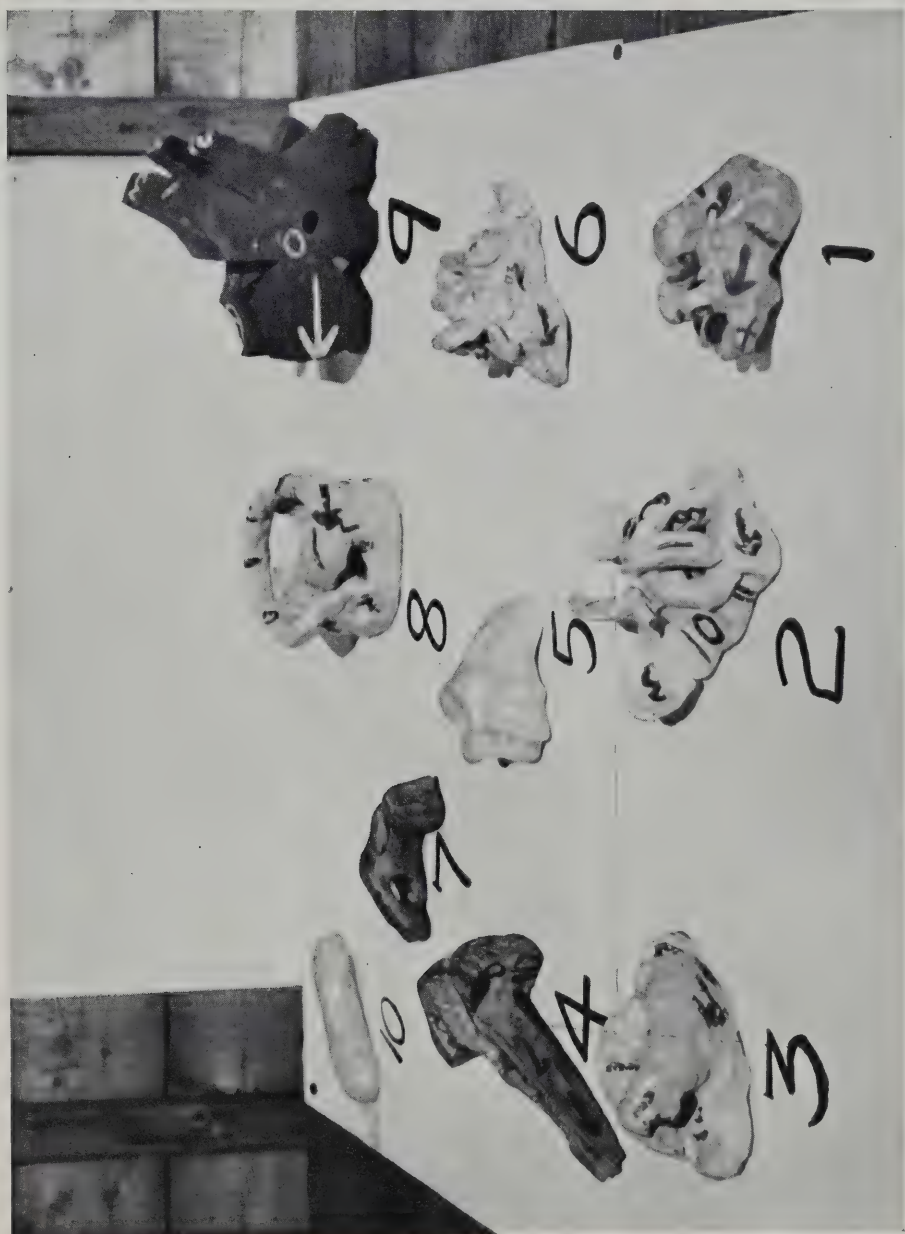
VII. CONCLUSION

Our experience would indicate that a projective technique involving the kinaesthetic and tactile modalities is practicable to administer and can evoke responses of clinical significance for the individual case. It is an aid to the more efficient use of the time devoted to the interview. The technique could be applied to many groups of different kinds, such as children at various stages of education, blind persons of different ages and stages of the onset of blindness, cases with lesions of the nervous system and various classifications of mental disorder. Further work would indicate if these groups could be differentiated as a class on the basis of the responses and observations recorded. In this preliminary study, and having regard to some inevitable variability in clinical diagnoses, attention was confined to major classifications.

We wish to acknowledge the help of Mr D. Graham and Mr W. Sluckin. Our thanks are also due to the Medical Superintendents of St Nicholas's Hospital, Gosforth, Newcastle-upon-Tyne, Cherry Knowle Hospital, Ryhope, near Sunderland, and St Mary's Hospital, Stannington, near Morpeth, for the facilities which enabled us to carry out the interviews and to Prof. A. Kennedy for initial facilities at the Newcastle General Hospital.

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(Manuscript received 20 August 1952)

SOME CRITICISM OF THE APPLICATION OF FACTOR ANALYSIS TO THE STUDY OF PERSONALITY

BY R. C. ALBINO

University of Natal, Pietermaritzburg

I. *General* (p. 164). II. *Taxonomy in science* (pp. 164–165). III. *The classification proposed by Eysenck* (pp. 165–166). IV. *The hypothesis that the population may be divided into normal and neurotic groups* (p. 166). V. *The hypothetico-deductive method* (pp. 166–167). VI. *The psychological continuum* (p. 167). VII. *Discussion* (pp. 167–168). VIII. *Summary* (p. 168). *References* (p. 168).

I. GENERAL

The work of Eysenck and his group at the Institute of Psychiatry in the University of London impresses both by its extent and by the confidence with which it is presented. It is implied by Eysenck⁽²⁾ (chapter I) that the methods of the group are truly scientific and that a taxonomy, as he defines one, is essential before any progress can be made in psychology. He also asserts that the methods of the dynamic and psycho-analytic psychologists are unscientific and are, by implication, unlikely to produce either useful or valid results.

Such implied claims must be carefully examined, for they are matters of opinion and not of fact, and if they are true a great deal of what passes for useful and valid psychological theory must be rejected. In fact, if Eysenck's views are accepted as they stand, the greater part of psycho-analysis, all theories based upon imprecise clinical observation and most of what passes for psychology amongst those who do not use either the refined methods of factorial analysis or precise experimental methods must be rejected.

This paper argues that some of the opinions held by Eysenck and his school are mistaken and that his method is not an example of the hypothetico-deductive method in the sense that he believes it to be. The findings themselves are not questioned for they are matters of fact, but it will be shown that some of the conclusions based upon them are little more than tautologies, and have no explanatory value.

II. TAXONOMY IN SCIENCE

To doubt the importance of classification for science would be foolish and, as Eysenck rightly remarks, no science could have advanced without a preliminary taxonomy. But he has not observed that the classifications that benefit science are of a special kind.

There are two logical methods of classification:

(a) The binomial, in which objects are placed in classes such that those in a class A possess a given characteristic, whilst those in a class B lack that characteristic. Both classes may be likewise divided by the application of another *fundamentum divisionis*, and the process may be repeated until the characteristics available in the objects are exhausted.

(b) The co-ordinate, in which objects are placed in co-ordinate systems such that an object is defined by co-ordinates $x_1, x_2, x_3, \dots, x_n$. Objects having identical co-ordinate values are said to belong to the same class.

There is no logical reason to prefer either method of classification, nor is there any logical reason for preferring any one of the many possible classifications of either form.

which may be applied to a given set of objects. To construct a set of classes it is only necessary to make an arbitrary choice of co-ordinates or characteristics from those which are available.

The selection of a preferred classificatory system depends upon non-logical criteria. If these there are two; the use to which the classification is to be put, or the acceptance of an hypothesis which demands a given classification. Fats are classified by cooks into edible and non-edible, according to the use to which they are to be put; chemists classify them according to their assumed chemical composition. On occasion the two types of classification may coincide. For example, meat and root vegetables form a utilitarian classification more or less equivalent to the chemical classification of carbohydrates and proteins. The coincidence is usually imperfect, as it is in the example given.

There are many examples in science of binomial and co-ordinate classifications and all illustrate the importance of non-logical criteria in their selection. A binomial classification was chosen by Linnaeus because it best fitted the hypothesis he held of the independent creation of distinct species. Such an hypothesis did not admit of transitional types between classes which a co-ordinate system would have included. The theory of evolution led to the rejection of the hypothesis of independent creation and removed the necessity for a rigid binomial classification. Now zoologists would admit transitional types between two species and accept a certain continuity between related species. In addition, certain biological classifications have been replaced by newer ones more in accord with evolutionary theory, even though they have no greater logical validity than the original systems.

The physical dimensions of length and mass and time are the clearest example of co-ordinate classification in science. These, however, were chosen after a distinction had been made by Galileo between primary and secondary qualities, and were preferred because the prior hypothesis that secondary qualities could be derived from primary was found to be true. Their continued use is a result of the value of that hypothesis in explaining all observed characteristics of physical objects. The early physicists could easily have chosen other co-ordinates such as colour, brightness, hardness, softness, etc.; that they did not do so was because such co-ordinates did not enable useful hypotheses to be made concerning the relation of one observation to another.

Thus, classifications, like factor patterns are indeterminate until either their uses are specified or until a prior hypothesis is stated which demands a given classification.

III. THE CLASSIFICATION PROPOSED BY EYSENCK

Eysenck proposes a co-ordinate classification of personality (2). There are two steps in his method of constructing such a classification:

- (a) He determines a set of factor patterns which will reproduce a correlation matrix derived from a set of test scores. These patterns are indeterminate (3).
- (b) He chooses from this set, by the method of criterion analysis (1), one which corresponds to an appropriate prior hypothesis. The prior hypothesis being one of the following; that the population may be divided into normal and neurotic, introverted and extraverted and normal and psychotic groups.

The first part of this procedure need not be criticized as it is an application of a valid mathematical argument. The second part is asserted by Eysenck to be an example of the

hypothetico-deductive method (2). This is only so in a narrow sense. The hypothesis is that certain test scores will be found to be associated with the population categories postulated (neurotic-normal, etc.). In showing this to be the case Eysenck has confirmed his hypothesis.

But this is not Eysenck's view of what he is doing. He asserts that he has demonstrated the existence of a neurotic-normal continuum in the population (or of continua of extraversion-introversion or of psychoticism-normality, whichever may be the case). There are two parts to this hypothesis: that the population may be divided into neurotic and normal groups; and that this division is not truly binomial, the characteristics of individuals varying continuously from the fully normal to the markedly neurotic. Each part must be separately considered in determining how far Eysenck is confirming his supposed hypothesis.

IV. THE HYPOTHESIS THAT THE POPULATION MAY BE DIVIDED INTO NORMAL AND NEUROTIC GROUPS

Eysenck does not confirm this hypothesis in any sense. He merely shows that certain scores on tests are related to clinically established categories. That is, he demonstrates the coincidence of two classifications; his own and that of the clinician. The original hypothesis is one made by the clinician that it is possible to isolate from the population a normal and a neurotic group of individuals. This is not properly an hypothesis but only a classification, and Eysenck is merely adding further descriptive terms to the two categories. He has not shown that the original classification is to be preferred in any way but has only demonstrated that it is logically permissible for a group of variables other than those originally used to isolate the categories. In the same way it could be shown that the trivial classification of youths into those with and without pimples coincides with a co-ordinate classification of the same youths on the basis of skin colour and amount of sebaceous secretion.

This criticism is not meant to deny all value to Eysenck's work, for he has accomplished the very useful task of converting what was originally a qualitative classification into a quantitative one.

V. THE HYPOTHETICO-DEDUCTIVE METHOD

Consideration of the nature of hypotheses shows further fallacies in Eysenck's argument. Popper divides hypotheses into *ad hoc* and genuine types (4). The former are those for which there is little evidence beyond the facts they are invoked to explain. A genuine hypothesis, on the other hand, always permits deduction of facts other than those upon which it is based. The hypothesis that group living results from a gregarious instinct is a *ad hoc* hypothesis, because we can deduce nothing from it except that animals of certain kinds tend to live in groups, which was the original observation.

The gravitational theory of Newton is a genuine hypothesis. From it we can infer both the facts it is invoked to explain—the movements of the planets—and an additional set of facts concerning the movements of the tides which were not in the original observation. Consideration of Eysenck's hypothesis of the neurotic-normal continuum shows it to be an *ad hoc* hypothesis. There is an observed dichotomy of individuals into neurotic and normal. Eysenck shows that this dichotomy can be related to test scores on certain tests. He therefore postulates a neurotic-normal continuum. But this is not a genuine hypothesis.

thesis; it is merely an amplified description of the two categories which were to be explained. To say that people can be put into the categories normal and neurotic because they lie on a neurotic-normal continuum is only a tautology.

However, the postulated classification of neurotic and normal subjects on a continuum does force a revision of the original binomial classification, and substitutes in its place a co-ordinate classification. This is an interesting finding and its implications must be examined.

VI. THE PSYCHOLOGICAL CONTINUUM

Eysenck's continuum may mean that the processes underlying neurosis and normality are continuous as well as that the symptoms are continuous. But this is not necessarily so, for temperature in a normal-typhoid series of patients is continuously distributed but nobody would nowadays assert that normal and typhoid patients are on a continuum with respect to disease. Like temperature, the tests used by Eysenck to establish the continuum are continuously distributed in the population; that is, each individual will produce a score of some magnitude and it is, therefore, a matter of necessity that their distribution should be continuous in a sample composed of normals and neurotics, and to have observed this fact is trivial.

Eysenck also argues that if there were no normal-neurotic continuum, but a simple qualitative dichotomy between the two types, then the tests discriminating between the normal and the neurotic groups would not intercorrelate in either the neurotic or normal groups taken alone. This is a less obviously fallacious argument in support of a continuum of causes as distinct from one of symptoms. But an illustration from physiology will clearly show that it is so. Temperature, water loss and respiration rate are correlated in normal subjects taken alone, and also in typhoid patients taken as a group and, also, the three variables are correlated between normal and typhoid groups. Nevertheless, normal and typhoid patients form a true dichotomy from the standpoint of origin.

The fallacy of Eysenck is to assume that continuity of changes of state in a system implies continuity of the antecedents of those changes. By the same argument a psychoanalyst would be led to believe that because there is a symptomatic continuum between the lack of conscience in the psychopath and the guilt of the melancholic the causes of both conditions should also lie on a continuum. But it may be that a discontinuous factor of cerebral defect lies at the psychopathic end of the symptomatic continuum, and it is indeed probable that this is the case.

Eysenck's continua probably imply no more than that he is investigating the functions of a single system—the nervous system. Though it may be the case that the causes of changes in function of this system are continuous, there is no reason to assume that this is so. Thus, the continua of Eysenck do not give us any information beyond the facts that they describe and are, therefore, not in any sense genuine hypotheses or explanatory principles.

VII. DISCUSSION

It is clear that Eysenck's application of factor analysis is only an example of scientific method in so far as he has made more precise descriptions of clinical categories. But those categories were isolated in the first place by the crude methods of the clinicians and were not originally the observations of the users of factorial methods. The question of whether the use of factorial methods was worthwhile depends entirely upon the theoretical

importance of the original categories. If the latter are found to be useful for the construction of hypotheses, or if they are demanded by certain hypotheses then the classifications of Eysenck are valuable.

Eysenck is fortunate because he has chosen some classifications which are clinically useful and valuable. But this is not a result of his method; it is a result of the work of others, and nothing in his method will aid in establishing valid and useful forms of classifications. With an appropriate series of tests one could establish continua of primary and secondary function, of economic and non-economic men, of aesthetic and non-aesthetic men, and of aggressive and passive men. In fact, one can describe an enormous number of dimensions of personality, but to do so would be to multiply non-significant entities beyond necessity.

In conclusion, we cannot controvert Eysenck's belief in the importance of hypotheses and taxonomy in science, but our search should be restricted to one for genuine hypotheses and confine our classifications to those of use or demand by hypotheses. Eysenck's main achievement has been the valuable but limited one of refining existing useful classifications. It has, however, few of the far-reaching implications for psychology he would believe it to have⁽²⁾. It is likely that the crude hypotheses of the Freudians are genuine hypotheses and explain many facts of behaviour. Attempts at rigorous confirmation of these hypotheses might be of greater value, both in producing a sound taxonomy of personality and establishing a theoretical foundation for psychology. The psycho-analytic hypotheses may be false; to show them to be so by experiment would be more scientific than to reject them out of hand and to put in their place a set of descriptions which explain nothing.

VIII. SUMMARY

1. It is argued that the methods of Eysenck are no more than techniques for refining descriptions. They do not establish the significance of the descriptions so made, and neither will they necessarily provide any genuine psychological hypotheses.

2. Eysenck's findings are not questioned, but their significance is doubted.

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(Manuscript received 17 November 1952)

THE APPLICATION OF FACTOR ANALYSIS TO THE STUDY OF PERSONALITY: A REPLY

By H. J. EYSENCK

Psychology Department, Institute of Psychiatry, Maudsley Hospital

Mr Albino's thoughtful and well-argued paper deserves a more detailed reply than I shall be able to give it here; many of the points which arise are discussed in my forthcoming book on *The Structure of Human Personality* (1953) and will therefore not be dealt with at any length in this brief note. I welcome Mr Albino's agreement with my stress on the importance of taxonomy in science generally, and in psychology in particular, as well as his recognition of the partly subjective nature of all dimensional systems in science; less sophisticated critics have often decried the importance of taxonomic problems as compared with so-called 'dynamic' ones, and have erroneously supposed that the subjectivity inherent in factorial solutions is not also found in the dimensional systems of physics.

Mr Albino's main criticism seems to be contained in the first paragraph of his *Discussion*. 'It is clear that Eysenck's application of factor analysis is only an example of scientific method in so far as he has made more precise descriptions of clinical categories. But those categories were isolated in the first place by the crude methods of the clinicians and were not originally the observations of the users of factorial methods. The question of whether the use of factorial methods was worth while depends entirely upon the theoretical importance of the original categories. . . . Eysenck is fortunate because he has chosen some classifications which are clinically useful and valuable. But this is not a result of his method; it is a result of the work of others, and nothing in his method will aid in establishing valid and useful forms of classification.' There are two main comments.

In the first place, history shows that in the early stages of any science common-sense observation and classification precede more exact scientific measurement. Heat and cold are distinguished before the thermometer is invented; bodies are observed to fall to the ground if unsupported, before the theory of gravitational force is enunciated. To compare large things with small, it would seem a little grudging to say that the inventor of the thermometer *only* made more precise descriptions of facts already known, or that Newton *only* succeeded in doing a little more accurately what everybody else was doing already! If indeed my work be judged to have succeeded in making more precise the description of personality, I would consider this praise indeed, rather than criticism. It is seldom realized to what extent *objectivity* and *measurement* aid in the development of science; in making common-sense or clinical observation more precise, they also furnish the means for correcting its errors and testing rigorously its underlying hypotheses.

There is one further advantage which objective measurement has over clinical diagnosis, an advantage which is so obvious, and appears to be at so elementary a level, that many psychologists fail to mention it at all. Scientists must obviously be able to designate their subject of study; the botanist must be able to recognize and identify trees, flowers and shrubs, and distinguish them from frogs, film stars and footballs. Much experimental work in clinical psychology is done in accordance with the simple schema of contrasting the performance of groups of schizophrenics and manic depressives, or hysterics and

anxiety states, on some test or series of tests. This presupposes that we are able to identify our 'subjects of study', i.e. schizophrenics, hysterics, and the like, with sufficient certainty to make the results meaningful. The contradictory results too frequently found in the literature suggest that this supposition is not justified, and the studies of reliability of psychiatric diagnoses summarized in *The Scientific Study of Personality* support the view that sources of error in diagnosis are so powerful as to make correct (i.e. reliable) recognition of the subjects of study practically impossible. If we can make bases of judgement more precise and more objective, then we would at least be able to identify members of groups for further study. It is difficult to see along what other lines we could proceed in trying to eliminate the contradictory findings of equally competent experimenters.

A final advantage implicit in this method is that it enables us to link up the abnormal, psychiatric field and its numerous theories and insightful discussions, with the large body of experimental studies in the normal field. Where there has been any attempt previously to link up the theories of Jung, Kretschmer, Kraepelin, Bleuler, Freud, and other psychiatrists on the one hand, and the experimental contributions of Heymans, Wiersma, Webb, Guilford, Thurstone, Wenger, Cattell, Freeman and other psychologists on the other, it has always been on a purely *semantic* level, based on argument from analogy. By undertaking precise and objective measurements, we make possible a direct comparison and link-up between these two great but distinct contributions to the psychology of personality.

So much for my first comment. The second one is rather more important scientifically. All through Mr Albino's paper runs the idea that I have taken some clinical classification (more or less at random, apparently, because he seems to think I was 'fortunate' in hitting upon a useful and valuable one) and made it more precise, but without adding anything to it. This is indeed an odd view to take, because my actual procedure was quite different, and I can only conclude that I have failed completely in my book to explain clearly and intelligibly the development of my theory. Perhaps an autobiographical note may help. When I entered the psychiatric field, I went through a number of psychiatric and psychoanalytical text-books in order to construct for myself some sort of orderly, systematic picture of the set of theories, hypotheses, nomenclatures, symptomatologies and classifications to be found there. I was looking, not necessarily for completeness or even truth, but I was looking for what must characterize every scientific system, namely consistency. But this I did not find. Even within the covers of a single text-book, I found syndromes treated as constituting *qualitatively* different mental states on one page, but as merely *quantitatively* different positions on a continuum on another. I found neurosis and psychosis treated as lying on two different dimensions on one page, but as different stages along one and the same dimension on another. I found Kretschmer's schizothymia identified with Jung's introversion on one page, only to find on another that Kretschmer groups the hysteric with the schizothyme, while for Jung the hysteric is the prototype of the extravert. In fact, there was such a lack of consistency that any experimental finding could be explained after the event, but none could be predicted with certainty before the event. The same lack of consistency which characterized psychiatric writings was also found to an equal extent in psychological texts, which, far from contributing anything new, were merely pale replicas of the psychiatric ones.

Under these circumstances, two courses of action are open to the investigator. One is

to start with a *tabula rasa*, and to disregard clinical description completely. This is the path followed, with important and suggestive results, by Cattell. The other is to take up hypotheses derived from clinical and psychiatric authors, state them in a form which makes possible an exact test, and carry out the necessary experiments and calculations. It is this path that I have followed, largely from a belief that it would be wasteful to throw out the baby with the bath-water, and to deprive oneself needlessly of the accumulated wisdom of centuries of sometimes brilliant, often acute, always relevant, clinical observation. I have therefore taken up such questions as the problem of quantitative or qualitative differences between clinical syndromes, or that of the relation between neuroticism and psychoticism, and submitted them to the best and most appropriate experimental test I could devise. The value of my contribution must stand or fall by the degree to which these tests are judged to be well designed, and, in particular, the degree to which the method of criterion analysis can be used to answer definitively questions of the kind I had in mind.

It is in dealing with Mr Albino's criticisms here that I have some difficulty. He makes two criticisms, the first of which applies to an argument I have never used, and the second of which misquotes completely an argument I do use. Mr Albino says that the fact that normals and neurotics show continuous distributions of test scores is trivial and is not relevant to the question of quantitative or qualitative differences; I agree, but having never used this argument at all am somewhat at a loss to know why it is adduced by Mr Albino.

The other argument, taken from the differences between normal subjects and typhoid patients, reveals a serious misunderstanding of the method of criterion analysis. Mr Albino says: 'Temperature, water loss and respiration rate are correlated in normal subjects taken alone and also in typhoid patients taken as a group and, also, the three variables are correlated between normal and typhoid groups. Nevertheless, normal and typhoid patients form a true dichotomy from the standpoint of origin.' Thus, apparently, the qualitative differences existing between normals and typhoid patients are not brought out by the statistical test, which gives the erroneous impression that the differences are merely quantitative. Extensive search having failed to reveal any trace of such correlations actually having been calculated, I must assume that Mr Albino has invented them in order to suit his argument, and has then based his argument on these hypothetical correlations which were invented precisely for the purpose of supporting it! Such circular reasoning does not appear to be a useful method of criticism. However, even if the facts should be as Mr Albino states them, they would be quite irrelevant to the methodological requirements of *criterion analysis*. What would be required would be two sets of correlations resulting in identical factors, and a set of criterion correlations *proportional* to one of these sets of factors. No doubt these could be invented as easily by Mr Albino as the original correlations, but I venture to predict that if the experiment were actually carried out (involving, of course, more than three items to be correlated), the results would not support the hypothesis of a quantitative continuum, but clearly indicate the essentially qualitative nature of the difference. In the absence of empirical data it is difficult to discuss this point any further.

Another point which I want to take up relates to Mr Albino's view that the work described in *The Scientific Study of Personality*, and presumably in *Dimensions of Personality* as well, is purely descriptive and not predictive or explanatory as is, for

instance, that of Newton. I would gladly agree that Mr Albino is absolutely right in this judgement; I would, however, venture to add that before we can formulate an explanatory theory to account for more elementary facts we must know what facts to account for. Newton was able to formulate an hypothesis taking in the movements of the planets and the falling of apples because for thousands of years precise though perhaps somewhat pedestrian determinations had been made of these movements. No such body of ascertained, indisputable facts is available in the field of psychology, least of all in that part of it dealing with personality; it follows that theory-making must either proceed *in vacuo*, or else must remain at the rather modest level at which I have kept it. Now, however, that we have succeeded in unearthing a large amount of factual material, it may be possible to go on to the formulation of more ambitious theories of the kind called 'explanatory' by Mr Albino, and indeed much work is in progress at the moment in attempts to verify and possibly modify such theories as have been suggested to us by our previous work.

One last comment. I have no wish to follow Mr Albino and assume the role of prophet, as when he maintains that 'it is likely that the crude hypotheses of the Freudians are genuine hypotheses and explain many facts of behaviour. Attempts at rigorous confirmation of these hypotheses might be of greater value, both in producing a sound taxonomy of personality and establishing a theoretical foundation for psychology. The psycho-analytic hypotheses may be false; to show them to be so by experiment would be more scientific than to reject them out of hand and to put in their place a set of descriptions which explain nothing'. Mr Albino has the right to his opinion, of course, but he is wrong in assuming that I reject psycho-analytic hypotheses 'out of hand'. My main reasons for not accepting them are: (1) there is no acceptable proof of any of them after more than 50 years of work in this field; (2) it is difficult to see how any crucial test can be carried out to disprove hypotheses stated so vaguely and in such imprecise terms that any factual result can be rationalized *ex post facto*. This is realized by psycho-analysts such as J. D. Sutherland, who says in his recent Presidential Address to the Medical Section of the British Psychological Society 'our hypotheses are not stated in a form which can be tested in a scientifically valid way'. It is not clear, then, on what grounds Mr Albino asserts that it would be more 'scientific' to show psycho-analytic hypotheses to be false than to try and build up a set of facts, and of explanatory hypotheses to account for these facts. Could it be that our notions of what constitutes science and scientific advancement are not in complete agreement? It would take us too far afield to discuss this point.

(*Manuscript received 24 December 1952*)

MEMORIZATION DURING RECALL

By E. C. POULTON

*Medical Research Council Applied Psychology Research Unit,
University of Cambridge*

I. *Introduction* (p. 173). II. *Experimental arrangement* (pp. 173-174). III. *Results* (pp. 174-175). IV. *Discussion* (pp. 175-176). V. *Summary* (p. 176). *References* (p. 176).

I. INTRODUCTION

The problem of dividing the attention was of considerable concern to the early experimental psychologists (e.g. see James (2)). Recently, it has again become important owing to the growing complexity of communications systems, which expose operators to information from two or more sources simultaneously (1). However, divided attention is a possibility which has been largely ignored in theories of human learning.

The results of a previous experiment (3) have shown that the active retention of the earlier items in a series can interfere with the memorization of later items. One possible explanation of this is the division of attention between recalling the earlier items in order to group them together, and memorizing the later items. The present paper has been written to demonstrate this detrimental effect of recall upon memorization which should be occurring simultaneously.

II. EXPERIMENTAL ARRANGEMENT

The experimental apparatus has been described previously (3). Each of 16 S's performed separately. He had to make two-handed 16-choice responses according to a random series of visual displays, projected one at a time upon a screen. His responses were recorded graphically.

Successive series of fifteen display items were presented. Each series was divided into three equal sections. The first five items, which will be called collectively the 'pre-display', had to be responded to as soon as they appeared. The next five items, the 'blackout', had to be learnt. While they were on the screen, the electric current to the response pens was turned off and a buzzer sounded. The pre-display and blackout items were presented at the same speed, either 1, 2, 3 or 4 sec. per item. The last five items, the 'post-display', were presented at the same or at a different one of the four display speeds. S was not told this speed in advance. While they were being presented, he first had to respond to all the blackout items which he could remember, in the order in which he had learnt them. He had then finally to respond to those post-display items, again in the correct serial order. This procedure is shown in diagrammatic form in Fig. 1.

Before the experiment S was taught the procedure, and learnt to respond while keeping his eyes on the screen. In the experiment he was presented with sixteen series, in each of which one of the four pre-display (or blackout display) speeds was combined with one of the four post-display speeds. A 16×16 Latin square design was used. There was a short interval between each series, during which encouragement was given but not knowledge of results. In scoring, the response record was fitted to the display series so as

to give as few errors as possible. Omissions were allowed, but the order of the responses was not altered.

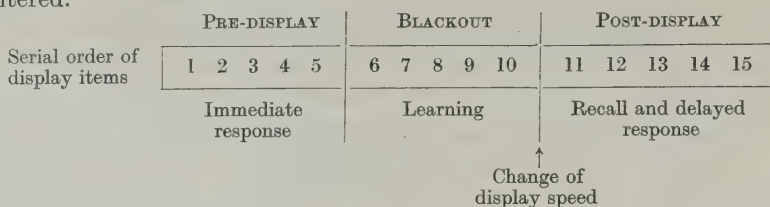


Fig. 1. Experimental procedure.

III. RESULTS

- (1) The five pre-display responses were practically 100% correct.
- (2) Variations in blackout display speed had no significant effect upon the percentage of correct blackout and post-display responses combined. This is shown in Table 1.
- (3) However a fast blackout speed significantly reduced the percentage of correct blackout responses, and significantly increased the percentage of correct post-display responses (see Table 1).

Table 1. *Effect of blackout and post-display speeds upon the percentage of correct blackout and post-display responses*

Display speeds. Blackout/ Post-display	Mean percentage of correct responses		
	Blackout	Post-display	Blackout and post-display combined
Fast/fast	34	66	50
Slow/fast	51	42	47
Fast/slow	41	94	68
Slow/slow	56	86	71

'Fast' means 1 sec. per display, 'slow' 3 or 4 sec. per display. For 5% significance, differences must be greater than between 4 and 12 (the exact value depending upon the reliability of the percentages concerned).

Table 2. *Distribution of correct blackout and post-display responses with the fast post-display speed*

		Post-display			
		0, 1	2	3	4, 5
Blackout	{ 0, 1	1	1	8	7
	2	4	6	9	6
	3	4	4	6	1
	{ 4, 5	5	2	0	0

χ^2 (for one degree of freedom) was significant at 1%.

- (4) Increasing the post-display speed significantly reduced the percentage of correct blackout responses; it much more markedly reduced the percentage of correct post-display responses (see Table 1).

(5) With the fast post-display speed, the number of correct blackout responses was inversely associated with the number of correct post-display responses. This is shown in Table 2. With slower post-display speeds the numbers were independent of each other.

(6) The fast post-display speed had its most marked detrimental effect upon the early post-display responses. This is shown in Fig. 2.

(7) Of the 64 series with the fast post-display speed, only five times did S's respond correctly to two or more blackout items, and also subsequently respond correctly to the first post-display item, although their eyes were directed at the display screen while responding. With the scoring method used, this is at about the level of chance for random responses.

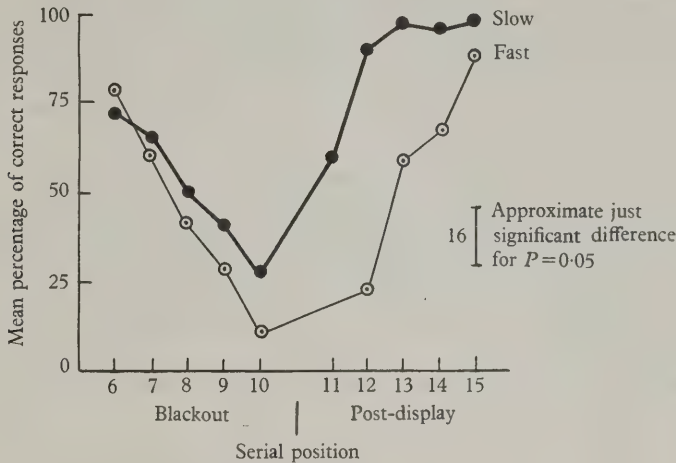


Fig. 2. Effect of post-display speed according to serial position of display item. The upper curve is for the slow post-display speeds of 3 and 4 sec. per display, the lower for the fast speed of 1 sec. per display.

IV. DISCUSSION

During the early part of the post-display period S was required to perform two tasks: to respond to the blackout displays, and to note the post-displays. With the fast post-display speed the two tasks had both to be performed in a very limited period of time. Results (4) and (6) (see Table 1 and Fig. 2) show that in these circumstances the average level of performance of both tasks suffered, although the early post-displays suffered most. However, instead of both suffering in a constant fixed proportion in each individual performance, result (5) (see Table 2) shows that there was an inverse association between number of correct blackout responses and number of correct post-display responses. And result (7) suggests that S never responded correctly to the first post-display item except by chance, if he was correctly recalling blackout displays while he was looking at it.

Clearly with the fast post-display speed S was confronted with conflicting requirements. He could either respond to the blackout displays, or note the post-displays. *He could not do both at once effectively.* Which he did was partly determined by the time allowed for memorizing the blackout items (result (3)). But this was not the only determinant, for the same inverse association between correct blackout and post-display responses was shown with both fast and slow blackout speeds.

The situation was similar in many respects to that of an experiment reported by Broadbent (1), in which S was required to listen to a new question while replying to a previous one. He concluded: 'we cannot attend perfectly to both the speech of others and to our own, but whether this is because we are attending to what we have just said or to what we are just about to say remains uncertain'. The results of the present experiment suggest that selecting and making the correct verbal responses are alone sufficient to account for his results, without postulating that S needs to listen to what he is saying.

Competition between recall and memorization suggests a mechanism by which the active retention of the earlier items in a series which is being learnt, could interfere with the memorization of later items (3). During the learning of any item, S may be devoting part of his attention to recalling and organizing the previous items. In which case the memorization of the item in question is likely to be less effective. This may well be the state of affairs even in learning lists of nonsense syllables from a rotating drum. For it is not necessarily correct to assume that because S looks at each syllable for a standard length of time and repeats it, his attention is necessarily fully occupied by it, when he knows that he will soon be expected to recall the previously memorized syllables.

V. SUMMARY

1. Presenting S with a rapid series of new items to which responses had eventually to be made, while he was attempting to respond to previously memorized items, affected both his responses to the memorized items and to the new items.
2. It was concluded that new items could not be memorized effectively while previous items were being recalled.
3. This suggested a mechanism which could account for the detrimental effect of active retention upon the memorization of new items.

I am greatly indebted to Prof. Sir Frederic Bartlett, F.R.S., for setting me the problem of memory during serial tasks; and to Dr N. H. Mackworth for providing the apparatus, and for helping me in the design of the experiment. Mr D. E. Broadbent suggested one of the methods of analysing the results. The S's were supplied by the Royal Navy. The work was carried out while receiving financial support from the Medical Research Council.

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(Manuscript received 10 September 1952)

CRITICAL NOTICE

BY AGNES CRAWFORD

Department of Psychology, University of Liverpool

A Further Study of Visual Perception. By M. D. VERNON. Cambridge University Press. 1952. Pp. xi+289. 35s.

Miss Vernon here undertakes a re-assessment 'of the material of her earlier book, *Visual Perception*' (Cambridge University Press, 1937), and 'of the mass of new experimental data' (p. 1). Besides that however, Miss Vernon says that 'It is desirable to state certain propositions as to the fundamental nature of the perceptual processes...[propositions which] emphasize what appears to be the most fundamental quality of the perceived field—its extraordinary unlikeness, so commonly overlooked in every day life, to the stimulus field.' The main proposition seems to be that we must recognize the 'amount of original construction which goes on in the ordinary process of perceiving'. Further, 'it is the author's belief and contention that the individual constructs his perceived world as far as possible in accordance with the maintenance of the maximum of stability, endurance and consistency', and 'strives to overlook much of the "variableness" in the physical world, and, further, to systematize his knowledge of such "variableness" along the lines of greatest invariability. The attempt to achieve this and at the same time to maintain an extreme sensitivity and the power of immediate response to all significant variation in the external world, seem to constitute the ruling principle of our perceptual construction' (p. 3).

In the second chapter, Miss Vernon distinguishes between the 'perceived field' and the 'stimulus field', thus: 'we know that we receive stimuli from the external world which vary continually in intensity, extensity and duration. Perceptual experience tells us that it is an orderly and continuous arrangement of solid and stable objects which do not change very greatly from one moment to another, or change only in certain known and predictable ways' (p. 7). The observer has to realize and differentiate between '(1) those changes which are unimportant and can be ignored, (2) those changes which are habitual, recognizable and easily predictable... , and (3) those changes which are wholly unforeseen and therefore must be examined in order that their "meaning" and significance may be understood.'

It seemed necessary to give this rather full account of Miss Vernon's aims and of her main propositions. These latter state in very general terms a theory of visual perception which is opposed to that put forward recently by J. J. Gibson in his book *The Perception of the Visual World* (Houghton Mifflin Co., 1950, 35s.), and one under which it is possible, as Miss Vernon shows, to include much psychological work that would not usually be found in a book with this title. The width of Miss Vernon's references and discussion is indeed impressive, and her efforts, justified on her own theory, to bring studies as diverse as those of Piaget, Michotte, Heidbreder and Lashley, as well as the work of Zangwill and others on brain injuries (to mention only some of the recent work she records) all under the heading of studies on visual perception would suggest that she regards perception (including learning) as the central, or possibly, as the integrating, process in both behaviour and psychological theory. This may very well be justified on any theory of perception, and indeed seems to be very much what Gibson is doing also. It does not, however, seem quite so justifiable to do this without any discussion at all of learning theory or of the differences between the Hull and Tolman schools where the part played by perception in learning is one of the points in dispute. It does, perhaps, indicate Miss Vernon's opinions on this dispute, however, that she later refers to the work of Fields, Lashley and some of the experiments on absolute and relative choice in the transposition experiments with animals.

In the succeeding chapters Miss Vernon deals with some of the old-established problems of perception. Chapter 3, called *The Perceptual Process*, is devoted to the work done on the fundamental stages in the process of perceiving, the perception of objects, the perception of representations of objects, the perception of abstract shapes and the perception of symbolic material. She states here that there seems to be little difference between the perception of representations of objects and the perception of objects themselves, and that consequently most of the experiments have been made with drawings of objects, rather than with real objects. This, though plausible and certainly more convenient for devising experimental material, has been taken too much for granted in the past, and could perhaps profitably be submitted now to experimental test. It seems possible that the slow increase which Binet found in the percentage of drawings recognized by children at various ages might contrast with the number of real objects which

these children might recognize and name correctly. It seems possible also that some of the difficulties about the perception of the third dimension might be the result, to some extent at least, of using two dimensional experimental material.

In this chapter, as in most of the others, two sorts of experimental work are described: first, experiments using tachistoscopic presentation, very restricted visual fields, and adult subjects, and secondly, the more recent experiments in much more natural conditions, using children. The latter are usually more fully described, and the results compared with those found in the earlier experiments with adults. Referring to the perception of words—the most important kind of symbolic material—Miss Vernon says that the perception of words is not completed till the meaning has been apprehended, though this seems to be ‘obtained immediately, without any awareness in ordinary reading of intermediate stages’. This surely applies only to words whose meaning is known, and not to words in a foreign language or to nonsense syllables.

In chapter 4, Miss Vernon outlines the work on the ‘Determination of Form’, the form quality of the stimulus, the influence of the nature of the parts upon the formation of the total structure and the demonstration of the Gestalt laws and principles. Chapter 5 continues with the older studies of two dimensional and three-dimensional spatial perception, with some of the newer work on the perception of the vertical and the estimation of distances in three-dimensional space, the studies of Piaget and Inhelder on the development of spatial perception in children, and some recent studies of the effect of brain injuries on spatial perception. Indeed, these four groupings of work can be found in the next chapter also, which deals with the constancies of shape, size, colour and brightness, though here Miss Vernon’s concluding sentence is somewhat discouraging: ‘With all the knowledge which has been gained of the phenomenology of the “constancies”, there is no satisfactory explanation of their method of operation’ (p. 148). The effects of what is called ‘the framework’ in psycho-physical experiments are discussed in chapter 7, and the functioning of this framework is interpreted as evidence for the proposition that one of the aims in perception is to maintain stability. The phenomena of apparent movement occupy most of chapter 8, though Miss Vernon herself, describing the few experiments on the perception of real movement, comments, justly, that this latter has received ‘curiously little attention’ until Michotte began to investigate it. Michotte and Piaget get the whole of the succeeding chapter. Michotte’s demonstrations of ‘the impression of reality’ are used to emphasize the difference between the stimulus field and the perceptual field, rather than, as they might well be, to suggest that objects themselves might give an even greater ‘impression of reality’ than Michotte’s shapes and outlines do.

It is very pleasing, however, to have such a full account of Michotte’s experiments, since relatively few copies of his book seem to be available in British libraries, and the book is out of print. Miss Vernon’s account is not full enough to be a substitute for Michotte’s own book, though it is sufficient to make one regret that the book is not more easily obtainable. I doubt if her picture of his ‘causality apparatus’ would be sufficient to enable even a good technician to make it.

After this refreshing excursion we return to questions of levels of attention, fluctuation of attention, reversible perspective and retinal rivalry, the division of attention between two tasks, and the effect of ‘set’ or ‘instruction’, or even of preconceived theories about perception on experimental results. There is, however, an account of some more recent work on emotional attitudes which is more interesting, notably that of Postman, Bruner and their colleagues, of Sherif, Luchins, Marks and others, and studies of the effects of rewards and punishments on perceptions, carried out by Murphy and others.

The material included in the book is very various; it is obviously a book to be referred to by those who want to know what work has been done on perception from about 1900 onwards, with some reference earlier than that. Miss Vernon says she has discarded some, though not all of the material in her earlier book. We may suppose that she has kept in the experiments where the results are based on adequate numbers of subjects and reasonably sound experimental methods, though little mention of these requirements is ever made, except negatively when she says that Piaget’s results need to be verified by experimenters using more subjects and more controlled conditions. Four sorts of work seem to be included under nearly every heading: old and new experiments of the classical kind, with tachistoscopic exposure or minimal illumination; new kinds of experiments, notably by Michotte, Postman and others; developmental studies of perception in children along with a few studies of animal perception, and studies of the perceptions of brain-injured adults and children. The descriptions of experiments, however, tend to be so condensed that it is not clear what the experimenters were doing, and sometimes so brief that they give a false impression of the results obtained. This is notably so in the brief sentence about Fields’s results on the perception of triangles in various positions by rats. It is not always clear which references are worth looking up if one is interested in any special problem of visual perception. A book which sets out explicitly to state and support some fundamental propositions about the perceptual processes should surely

give a sufficiently detailed account of some representative experiments which support or do not support the propositions, to enable the reader to assess the value of that support or to assess the value of results which do not support the propositions.

Miss Vernon adds an Appendix commenting on Gibson's *The Perception of the Visual World*. She begins by saying that the book expounds a theory 'extremely opposite' to her own, outlines his theory very clearly and adequately, and says that she agrees with much of what he says, though she cannot see the force of all his criticisms and disagrees with his estimation of the value of the Gestalt theorists' work on visual perception. Both books cover rather surprisingly similar material; the main difference seems to be in the point at which they place 'learning' in the perceptual processes. Miss Vernon wants to place it at the beginning—the ordering of sensations or 'construction of perceptions'. Gibson discards sensations, and maintains that perceptions are given as ordered, and that learning comes in, much later, either in 'modifying' the perceptions, or in enabling them to be interpreted. He maintains that there may be a few 'embryonic perceptions' with 'embryonic meanings' against Miss Vernon, who would not allow this, though she is nearly forced into it by Michotte's results on the perception of causality. Sometimes Miss Vernon seems to be saying just what Gibson says, especially when she talks about our perception of the perceptual field as objects, but then rather shies away from that because of her awareness of the 'extraordinary unlikeness' between sensations and perceptions, and because of her awareness of how percepts may be modified both in experience and in recall. Further, Miss Vernon is more interested in individual differences in perception than Gibson is, and emphasizes that these must be accounted for in any theory of perception. Gibson would allow them to be accounted for by the distinction he makes between 'literal perception of the visual world' and 'schematic perception of the visual world', and this seems a valid distinction; further it is one which would largely resolve this part of the theoretical difference between these two expositions, since 'schematic perceptions' are perceptions modified by past experience, interests, and emotional attitudes.

However, the attempt which is made in the first two chapters to relate conceptual thinking, abstraction and generalization to visual perception, to show the relation to perception theory of Piaget's work, Michotte's demonstrations and the studies of the effects of brain injuries, is a valuable one and a book which provokes readers to disagreement is more useful than one at which readers simply nod approvingly. As befits a book on visual perception, there appear to be very few misprints, though I am still in some doubt whether 'opposite' is a misprint for 'apposite' in Appendix A.

CORRESPONDENCE

THE EDITOR,
British Journal of Psychology

Department of Psychology,
Maudsley Hospital,
Denmark Hill,
London, S.E. 5

6 November 1952

Dear Sir,

I believe that most members of the B.P.S. would agree that the primary function of a review in our journals is to portray objectively the content of the material reviewed and that this portrayal should not be seriously affected by the personal views of the reviewer. I feel that this expectation has not been fulfilled by K. R. L. Hall's recent review of the *Scientific Session on the Physiological Teachings of Academician I. P. Pavlov* in the August 1952 issue of the General Section of the *British Journal of Psychology*.

The reader of the review will not find any indication of the claims of scientific advance which are being made by Soviet physiologists. For example, one report refers to the demonstration of the influence of experimental neuroses and chronic overstrain on the genesis of tumours, both benign and malignant. Another report is to the effect that disturbances of internal organs can be produced by experimentally induced breakdown of higher nervous activity and that these conditions can be successfully treated, and so on.

Aside from the ignoring of such claims, I feel that the general atmosphere of the discussion has been seriously misrepresented. One would not obtain the impression from the review that appeals were made by all the leading participants for the fullest freedom of criticism, regardless of status or authority of the individuals concerned, because such freedom was regarded as necessary to the development of science. The review gives the impression instead of a dogma-bound discussion in which the word 'heretical' was one of the main features and which was characterized by the fact that the theories and methods of Sherrington have been completely discarded in Soviet physiological literature as a whole.

I have not in fact been able to find the word 'heretical' in the book reviewed. What is more important, however, is that Sherrington, far from being rejected, appears in current Soviet literature as one of the mainstays of neurophysiology. Perhaps the following quotation (kindly translated by Dr L. Crome) from Bykov, one of the main participants in the session, will help to place this question on a factual basis. He says: 'Millennia were required for the formation of correct methods for the study of all forms of behaviour of a complete organism and before an opportunity arose to interpret the most complicated reactions of animals by means of a clear and precise physiological conception—the reflex.'

'The credit for this achievement belongs to two representatives of science—Sherrington, who interpreted the elementary reactions which take place with the participation of the spinal cord, and Pavlov who presented the possibility to approach the study of the higher forms of reflex activity, of the cortical reflexes, by means of the conditioned reflexes method.' (K. M. Bykov, 1947, *The Cortex and the Internal Organs*, p. 258.)

It is in the latter field that Sherrington's views are regarded as inadequate by Soviet physiologists.

I do not doubt that many British psychologists would find the atmosphere of the reported discussions difficult to understand and that they would disagree strongly with many of the theories advanced. It can do nothing but good to our science to give the fullest expression to such difficulties and disagreements on the basis of an objective evaluation of the available material. We can, however, do ourselves a great deal of harm if our writings merely contribute to the perpetuation of national and political stereotypes, especially in the present international situation. On the one hand it serves to make us neglect the possibility of important trends in the development of Soviet science. On the other hand we run the risk of convincing our Soviet colleagues that we are so affected by political feelings that we cannot make an objective evaluation of scientific questions.

It is in view especially of this last consideration that I would appreciate the publication of this letter in the *Journal*.

Yours faithfully,

M. B. SHAPIRO

THE EDITOR,
British Journal of Psychology
19 December 1952

Dear Sir,

I think perhaps it is necessary to point out that, in reviewing for a journal of psychology a publication such as the *Pavlov Session*, the reviewer's main task might be thought to be that of abstracting and evaluating critically those main trends that are most relevant to psychology. It is for that reason that the Pavlov Session was discussed in relation to London's *Contemporary Psychology in the Soviet Union*. It is for that reason, further, that in a short review, detailed reference to 'claims of scientific advance', two of which are mentioned by Mr Shapiro, is scarcely feasible or desirable, particularly where many of these investigations are much more the concern of experimental medicine and pharmacology than of psychology. The material under discussion is, as it were, somewhat combustible, and one could, no doubt, have adopted an attitude of blinkered objectivity by ignoring the insistently repeated context in which the detailed references are set.

I should like briefly to recapitulate the themes of the Pavlov Session. These are:

(1) In the opening address: Pavlov's teaching provides a scientific basis for the creative development of physiology, medicine and psychology, and the shortcomings in developing his work must be eliminated as quickly as possible.

(2) In Vavilov's Presidential address: Soviet physiologists are taken to task for having 'deviated considerably from Pavlov's teachings. There have been attempts—not too frequent, happily—at an erroneous and unwarranted revision of Pavlov's views.' It is this deviationism, by Orbeli, Anokhin and others, who have chosen to develop their own lines of research, and, in doing so, have criticized Pavlov, which is repeatedly criticized by the speakers in this session. To say that one has 'misinterpreted' the general atmosphere of the discussion by drawing attention to its undoubted unity of purpose—exhortation to adhere to Pavlov theory—is ludicrous. To say, further, that 'the fullest freedom of criticism' was advocated by the leading speakers is somewhat ingenious in view of the fact that such criticism will obviously be accepted only within the assigned sphere of orthodoxy. I have no doubt that dull and unimaginative Pavlovians are severely

criticized by their more acute and adventurous fellow-Pavlovians, but what of the non-Pavlovians like Orbeli and Anokhin? There seems to me to be only one degree of freedom for them—self-criticism and retraction.

A further point Mr Shapiro makes is that 'Sherrington, far from being rejected, appears in current Soviet literature as one of the mainstays of neurophysiology'. He then quotes the comments of Bykov written in 1947. I have no doubt at all that many Soviet physiologists, as individuals, do very highly esteem the work of Sherrington, and previously have been allowed to say so. But the Pavlov Session took place in 1950, and Mr Shapiro would probably agree that very radical changes of opinion can be brought about in the U.S.S.R. in even shorter periods. The 'official' view expressed by Vavilov is that Sherrington is one of the protagonists of the 'open or concealed opposition' to Pavlov's theory that, he says, has always been encountered in the bourgeois countries. Sherrington's 'dualism' is condemned, as is Orbeli's use of the subjective, or psychological, as well as the objective or physiological, approach to the study of the organism. Sherrington's 'concept of integration' is rejected, and Anokhin is criticized for his preference for this concept and for accusing Pavlov of 'isolation' from foreign neurology. Pavlov's materialist teachings, says Razenkov, must be defended 'against the reactionary assaults of men like Sherrington, Lashley, Fulton and other Western idealist physiologists'. So long as Sherrington has this apparent status of Physiological Enemy Number 1, it seems unlikely that his work will be regarded in the U.S.S.R. as one of the mainstays of neurophysiology.

I should like to revert briefly to the point Miss Vince made in her letter (this *Journal*, February 1953) as to the importance of Pavlov's idea of the second signal system. According to Ivanov-Smolensky (*Pavlov Session*, p. 86), it was when, late in life, Pavlov turned his attention to clinical work 'that there arose in his mind the remarkable idea of a first and second signal system of the cerebral cortex'. From other sources available in English, it seems that Pavlov connects the idea of varying inter-relationships between the first and second signal systems with his general conception of 'types' of nervous system derived from observations of his dogs. For example, in a paper given at the Neurological Congress in London in 1935, he is quoted by Frolov¹ as saying: '... thanks to the two signalling systems... the mass of human beings can be divided into artistic, thinking and intermediate types. The last-named combines the work of both systems in the requisite degree' (p. 233). In hysterical persons, he describes how 'general weakness naturally has a special effect on the second signalling system, which in the artistic type in any case yields pride of place to the first system... Hence arises a chaotic character of the activity of the first signalling system and an emotional background in the form of pathological fantasies and impulsive emotionality with profound destruction of the general nervous equilibrium...' (p. 234). Again, in a paper on 'Obsessional neurosis and paranoia',² Pavlov uses his idea of the two signalling systems to describe different degrees of intensity of 'pathological inertness' which may take place 'either in cells receiving immediate stimulation from external as well as internal agents (first signalling system of reality), or in different cells (kinaesthetic, auditory, visual) of the verbal system (second signalling system)'.

¹ *Pavlov and his school* (1937). London. Also Chapter LVI and passim. *Lectures on Conditioned Reflexes* vol. II, 1941.

² *J. Ment. Sci.* (1934), LXXX, 187-97.

By introducing his second signal system, Pavlov attempted to extend his conditioned-reflex theory to cover the higher nervous activity of human beings and to give systematic explanation for psychopathological changes in personality. From the evolutionary, developmental aspect, this was no doubt a consistent and logical step for him to have taken. In his role as 'psychiatrist of the future', assigned to him by H. S. Liddell, this was a necessary step, yet it remains, so far as I can judge, an unimportant one, partly for the reasons I have already given in replying to Miss Vince's letter, partly because the valuable contributions to the study of language and thought that have been made in neuropsychiatry and in developmental psychology seem to have occurred quite independently of this theory. Almost all the work on the higher mental processes in human beings, on concept formation, abstraction, on perception in relation to personality factors, and so on, could be described as involving the inter-relationships of the first and second signal system, yet, fortunately I think, this work has proceeded without reference to this late addition to conditioning theory.

Yours faithfully,
K. R. L. HALL

PUBLICATIONS RECENTLY RECEIVED

Psychosurgical Problems. Edited by FRED A. METTLER. London: Routledge and Kegan Paul Ltd. 1952. Pp. xii + 356. 42s. net.

This is the second report of the Columbia-Greystone Associates on the effects of surgical methods of treatment of mental disorder. (The first appeared in 1949.) The procedures employed (and well described in this volume by Dr Lawrence Pool) were bilateral ligation of the superior cerebral veins, thalamotomy, topectomy, transorbital leucotomy and thermocoagulation of the cerebral cortex in the rostral portion of the frontal lobes. A carefully selected group of thirty patients (all schizophrenic) provided the basic data; there were six control cases. As in the earlier study, extensive and intensive physiological, psychological and psychiatric investigations were undertaken by teams of specialists and the results assembled in successive chapters. Drs Mettler and Landis present the general conclusions in a penultimate chapter. The report closes with a short follow-up account of the patients studied in the original (1949) investigation.

By and large, the results of this inquiry are almost wholly negative. At the physiological level, no significant alteration in sensory, motor or autonomic responses could be established post-operatively, apart from minor changes in response to glucose tolerance tests. At the psychological level, changes following operation were in general transitory and non-specific. In particular, no permanent intellectual defect could be ascertained in any patient. At the psychiatric level, the incidence of post-operative improvement in this particular group of patients was very low, and attempts to validate certain prognostic criteria were uniformly unsuccessful. As the authors point out (p. 311), if the methodology of their work is admitted to be correct, it is impossible to avoid the conclusion that the answers to the questions which the research formulated are truly negative. No reader of this volume is likely to disagree.

The upshot would appear to be either that the psychosurgical procedures under inquiry are without effect (at least on schizophrenics), or that this particular team of investigators asked the wrong questions. The former alternative is improbable (though perhaps less improbable than is sometimes supposed); the latter might seem to some exceedingly likely. To the present reviewer, at least, it appears most unlikely that an acceptable rationale for psychosurgical procedures will ever result from a frontal assault upon the characteristics of patients submitted to operation. It must be clearly understood that scientific method in psychiatric research is by no means necessarily synonymous with meticulous quantitative analysis of the changes (if any) brought about by operation in any particular group of patients. (Indeed the present book constitutes a grim object lesson in the futilities of this approach.) If the *modus operandi* of psychosurgery is ever to be understood (and improved methods of surgical treatment to develop) this can come about only through advances in our basic knowledge of psychiatric disease processes on the one hand, and a clearer understanding of cerebral localization of function on the other. Unless the problem is regarded in this light, it would seem that psychiatric patients (to quote Prof. Bleuler) 'are threatened with all kinds of useless surgical trials'—and, one may add, psychiatric investigators with all kinds of useless research projects. One might therefore recommend the Columbia-Greystone Associates to take a vacation from the hospital wards and to devote themselves (for many years at least) to fundamental research in their respective laboratories.

O.L.Z.

Struktur und Metrik figural-optischer Wahrnehmung. By DR E. RAUSCH. Frankfurt am Main: Verlag Dr Waldemar Kramer. 1952. Pp. xiv + 404. DM. 18.

This book represents the result of investigations started in 1938. It deals not only with the so-called geometrical-optical illusions, but includes also the wider problem of perceptual structure in the field of vision. The author is a follower of the Gestalt school and he uses its main principles to explain the phenomena under investigation.

He attaches great importance to 'prägnanz', i.e. to the forms which can be considered as 'best', 'perfect', 'ideal', in comparison with others. A straight line in the horizontal or vertical position is such a form, as is a figure which may be derived from the orthogonal intersection of two extensions lying in the horizontal and vertical positions. From this we may deduce an 'orthogonality' principle. It is on this principle Prof. Rausch bases his explanation of the observed phenomena.

In the field of vision some figures look as if they were derived from 'prägnanz'-figures, as if deflected

(*verzerrt*) from the main directions of those figures. In perception those figures manifest a tendency to appear nearer to the position of a corresponding 'prägnanz'-figure (*entzerrt*) than they are in 'reality'. In the field of vision some 'forces' are engaged, initiated by those 'prägnanz'-forms or -positions; the parallelograms look as if their two extensions were perceived nearer to the 'orthogonality'-position, i.e. to the rectangle.

If we take a system of co-ordinates based on the orthogonal intersection of horizontal and vertical lines (Descartes' system) we can distinguish two main positions of the parallelograms: (1) the position where their sides are judged to be parallel or nearly parallel or related to one of the lines of the system; (2) the position where the diagonal is parallel or nearly parallel or related to one of the lines of the system.

Judgements of the first kind occur more frequently because here the parallelogram appears in a steady, solid position whereas in the second case the position appears to be more unstable. Prof. Rausch carried out numerous experiments using squares, rhombs, rectangles and other parallelograms of various shapes and in different positions. He measured angles, directions, lengths of sides and diagonals, points of division and so on. The results obtained indicated, in Prof. Rausch's opinion, that there was a tendency to change the perception of different parts or features of figures in the direction of orthogonality. He includes in his studies many of the well-known geometrical-optical illusions (those associated with the names of Poggendorf, Zöllner, Hering, Sander, Lipps and Müller-Lyer) and finds for them the same explanation.

Apart from the kind of general objection which can be directed against Gestalt theory as a whole the following criticisms may be levelled against Prof. Rausch's conclusions.

(1) While his orthogonality principle may account satisfactorily for the examples which he uses there are other visual phenomena to which he does not refer and which do not comply with his findings.

(2) Prof. Rausch does not indicate that he controlled the fixation of his observers during the course of an experiment. Many workers have shown that changing the fixation point has considerable influence on the structure of perceived figures.

(3) Professor Rausch did not try to establish any relationship between the perceived structures and the physical-optical properties of the images formed on the retinas of his subject. It is probable that this would have given a more useful kind of insight into the observed phenomena than is obtained by using words like 'tendency' or 'force', words which are in this context nowhere properly defined.

F. L. ZAJĄC

The Measurement of Hearing. By I. J. HIRSCH. New York and London: McGraw-Hill. 1952. Pp. ix + 364. \$6.00 (51s. net).

This is a useful and timely book. The application of electronic resources to auditory research has not solved all the problems of hearing, but has produced, in a short time, a quantity of data and special techniques of great potential usefulness in a wider field. Increasingly to-day, account is being taken of conditions leading to difficulty in auditory communication, and of the adaptive reactions of the individual to such conditions. This is especially, though not exclusively, so where the cause of the difficulty lies in deafness. In the detection, assessment and treatment of the deaf, methods are in process of enlargement upon a basis of the fresh knowledge now to hand. This book, unencumbered by pathological and clinical material, or by the minuter technicalities of auditory theory, lays out with admirable clarity the basic information at present available about psycho-physical aspects of hearing and hearing-loss. It indicates the bearing of this information on diagnosis and treatment.

Introductory chapters on the physics of sound and of electro-acoustic systems are followed by a discussion of pure-tone audiometry. This takes the reader, as it should, beyond the established practical techniques. Stimulus-factors upon which the intelligibility of speech depends are next dealt with, and this leads on to a discussion of speech audiometry. Masking and fatigue are discussed in some detail, and the relation of the frequency-spectrum of a masking noise to its effects are made clear. The emphasis given to 'white-noise' masking is valuable. Further chapters deal with differential sensitivity, loudness and recruitment, binaural hearing and bone conduction. The chapter on conditioning in relation to audiometry may be found useful by those interested in special techniques applicable to young children. A final chapter on clinical audiometry further discusses the bearings of the material reported on the practical problems of hearing-loss assessment.

It may be hoped that the book will be of considerable service to many kinds of people engaged in handling the deaf. It brings together for the first time, in a digestible form, a quantity of matter which

was previously not easily accessible. Those whose interests lie in other fields of auditory perception will also find much of value in it. Indeed perhaps the chief criticism of the book is that its scope was not made wider so as to cover the whole field of auditory communication under conditions of difficulty.

A Text-book of Human Psychology. By PETER MCKELLAR, with a Foreword by D. W. HARDING, London: Cohen and West Ltd. 1952. Pp. xv+384. 30s. net.

A new text-book, written and published in this country, is likely to be read eagerly by those responsible for teaching psychology, in the hope of finding a book which combines an adequate presentation of the subject with cheapness and availability. This is especially true for books which aim, as this one does, at a more advanced treatment than the usual 'Introduction'.

In some respects, Dr McKellar has succeeded in producing an extremely interesting and valuable addition to current text-books. As Prof. Harding points out in his Foreword, although the main body of work quoted must necessarily be American, in this book much more attention than is usual in text-books is paid to the earlier thinkers in the 'English tradition'. Shand, in particular, is given very sympathetic treatment. This, in Dr McKellar's hands, gives the book a most pleasing sense of historical continuity and helps it to avoid the breathless cataloguing of many texts, which appear to be in haste to be published before becoming out of date. Style, and the method of treatment of detailed problems, combine to make this book easy and pleasant to read.

It is, perhaps, inevitable that the book fails in its aim to give comprehensive treatment of the subject at a reasonably advanced level. So many have tried and failed in this aim that it seems likely that the time is approaching when psychologists will need to follow the example of the more established sciences and use texts on particular aspects of their subjects except at the most elementary level. This book, however, has the great value of being strongest where most other texts are weakest, and thus forms a most useful complementary volume to the better American texts. Dr McKellar is at his happiest when discussing such topics as 'The Motivation of Behaviour', 'Emotions', 'Systems of Emotional Organisation', 'Unconscious Mental Life' and so on. In these chapters, evidence from many sources is marshalled and the problems discussed in a lively and scholarly manner. There is little doubt that most students will find these chapters most valuable. The chief fault to be found with them is the use, as examples, of numerous personal anecdotes and of a wealth of literary allusions, from Aristophanes to Dostoevsky. Such examples, while adding colour to a lecture, seem unconvincing in print.

The book is less good in the more 'cognitive' fields. The chapters on learning and remembering, especially, seem rather pedestrian, and are certainly less adequate than comparable chapters in many of the better American texts. This seems in part to be due to an attempt to cover too much ground too quickly. Twenty-eight pages is surely not sufficient to range from a discussion of the learning situation, through the theories of Thorndike, Guthrie, Tolman, Hull and Mowrer to such complexities as social learning? This haste produces a somewhat sketchy treatment which is certainly not at the advanced level, and occasionally leads the author into verbal carelessness which is undesirable in a text-book. For example, the basic formula for conditioning experiments is given as



This could easily be interpreted as implying a reversal of the normal temporal relationships between S and S.1. In view of the difficulty students experience in being accurate about temporal gradients in conditioning, such careless phrasing could be most misleading. Similarly, the subsequent discussion adopts a pure stimulus-substitution interpretation of conditioning with no indication of the difficulties in such a view. A second-year text should show more sophistication on such points. The treatment of perception is also disappointing. This chapter appears to be lacking in perspective. There seems little to be gained by classifying the Duplicity theory as a General theory, and the Young-Helmholtz and Willmer theories as Specific, as examples of different levels of scientific theory. Would it not also be an unusual second-year student who was acquainted with the 'alternative theories of colour vision' of Willmer, Hartridge and Granit, and yet needed Von Kries's Duplicity theory explained to him in words of one syllable?

This book does not solve the problem for the instructor of being forced to recommend particular chapters

from a number of texts, with its attendant difficulties for the limited means of students, but it does supply chapters on conative, orective aspects which are better than most. For this, it will be a useful addition to teaching texts.

G.C.D.

The Lonely Crowd: a Study of the Changing American Character. By DAVID RIESMAN, in collaboration with REUEL DENNEY and NATHAN GLAZER. New Haven: Yale University Press; London: Geoffrey Cumberlege. 1950. Pp. xviii + 386. 25s. net.

It is refreshing to find a book on social psychology that has been written for adults, without a constant eye to the needs of undergraduate students. In arguing a theme of serious importance to those who live in large-scale western communities, pre-eminently America, Prof. Riesman declines to limit himself to the findings of strictly scientific inquiry (though he uses what are available) and draws on common knowledge and observation of politics, the press, radio and films, and in fact the ordinary world. In this he may be going beyond the range of many psychologists. They will if they wish be able to ignore his book by pointing out that he relies largely on the subjective interpretation of material gathered by his own selective observation and undoubtedly influenced by his hypotheses.

The hypotheses are so interesting that it would be a pity to do this. He suggests that from having been in the nineteenth century 'inner-directed' (by standards, ideals and discipline implanted early in life and felt by the individual as his own) most Americans have now become 'other-directed', taking standards and guidance mainly from their contemporaries at each stage in their lives. 'The goals toward which the other-directed person strives shift with that guidance; it is only the process of striving itself and the process of paying close attention to the signals from others that remain unaltered throughout life.' In a very interesting discussion of education from this standpoint the author suggests that the tendency for children 'to learn democracy by underplaying the skills of intellect and overplaying the skills of gregariousness and amiability' is paralleled in industry by an increasing concern with co-operation between people rather than with technical skill. 'Thus the other-directed child is taught at school to take his place in a society where the concern of the group is less with what it produces than with its internal group relations, its morale.' From this angle Prof. Riesman offers a discursive and stimulating survey of American attitudes to politics, business, popular culture, leisure and sex, his general standpoint being well suggested by the last sentence of the book. 'The idea that men are created free and equal is both true and misleading: men are created different; they lose their social freedom and their individual autonomy in seeking to become like each other.' The book suffers by its relative lack of objectively assembled facts, and its views invite more disciplined testing, but social psychology would be a far more interesting subject if there were more books as stimulating and adult as this.

D.W.H.

Faces in the Crowd. By DAVID RIESMAN. New Haven: Yale University Press; London: Geoffrey Cumberlege. 1952. Pp. xii + 751. 32s. 6d. net.

The Lonely Crowd suffered from a lack of adequate documentation. *Faces in the Crowd* to some extent makes good the defect, though the author disavows any attempt to 'prove' his earlier generalizations by the material offered here. The very full account of interviews and the detailed studies of individual character and outlook which this book provides serve at least to illustrate and clarify the types of character formation suggested by Prof. Riesman. Although the interviewers are said to have been mostly untrained and had to use a very long list of set questions, and although the method of the interview is inadequately indicated, it is still the direct reports of the interviews with twenty-one individuals that form the most interesting part of the book. They give a terrible picture of the nullities and confusions of a mass civilization. Prof. Riesman's lengthy interpretations of each interview seem over-elaborate and laboured, and the book is weighed down with repetition. The nature of the inquiry, like many in social psychology, creates a formidable problem in the presentation of the findings, a problem which has defeated this book. Yet there are some illuminating insights and stimulating suggestions lurking amongst the wordage, and the psychologist who has found *The Lonely Crowd* valuable will discover useful amplification of the theme in this later book.

D.W.H.

The Psychology of Adolescent 'Development'. By RAYMOND G. KUHLEN. New York: Harper Brothers. 1952. Pp. xvii + 675. \$5.00.

The danger besetting psychologists writing at the moment, and not only in America, arises out of the recent redefinition of our science. 'The study of human behaviour' has beckoned its practitioners in many directions at once, in order to 'see man in the total field of forces' and 'appreciate the multiple causation of human action'. The result too often is the hypertrophied text-book, although we are attempting to deal with a limited field in a manner appropriate to students of limited psychological training.

Even in a systematic and clearly written book like this, the psychology of adolescence easily becomes embedded in a mass of select and simplified reportage from developmental physiology, anatomy, cultural anthropology, sociology, economics, politics and history. These studies are, indeed, a vital necessity for the proper education of psychologists of adolescence. The last thirty years has provided just such cross references essential to restrain any previous over-weening pride in the all-importance of 'psychological motives'. But surely now that we have been chastened and corrected we should try once more to say simply and clearly what *psychologists* still have to say about problems of human behaviour? To do this we must become jealous to establish the necessary limits of practical specialisation in our science.

Prof. Kuhlen tells us that he writes for a typical (American) College or University class which includes 'home economics majors, nurses, prospective teachers' as well as 'psychology majors'. As he himself insists, it is difficult to construct a text appropriate to such diverse needs and one may reasonably doubt even at this distance, whether he has succeeded in his intimidating task which seems to be that of inculcating in his mixed class by means of a single text, a general social-scientific outlook as well as a special understanding of this specific problem. Certainly this book, so far as the writer is aware, meets none of the needs of special courses in British Universities, which would usually be superimposed upon a more homogeneous background of basic psychological theory.

It is, however, a useful text for our libraries even at present prices, especially perhaps in Educational Departments. There, its applied approach and wealth of school-experimentation would be most appreciated. The applied educational psychology of adolescence is certainly the best part of the book, which does contain a great deal of interesting, unusual and up-to-date material and certainly shows a theoretic outlook on the whole balanced and shrewd, although the introductions to certain subjects, such as Intelligence, seem inadequate and over-simplified. Throughout the book also one meets evidence of suggestive and imaginative thinking as, for instance: 'It would seem that at least some of the problems of adolescents are created by parents' preoccupation with their own problems, or by their inability to appreciate their children because of their own ageing process', or, 'It seems likely that research on adolescence [sc.—in U.S.A.] has yielded mainly a "psychology" of the urban middle classes.'

Prof. Kuhlen, despite his good judgement, seems ineffectively nervous of the important task of assessing the contribution of depth psychology to the understanding of the typical adolescent problems—sex, family status, and social status. We shall not be able to control the impact of Freud on our intelligent students by attempting to give the impression by silence that psycho-analytic clinical experience is of no importance here.

But when all is said, Prof. Kuhlen establishes an argument on the whole sound and important: 'It is believed that adolescent development and the adjustment problems that typify adolescence can best be understood if they are examined from the point of view of biological change in a cultural context and are placed as squarely as possible in the perspective of the developmental trends characterizing childhood and (where research evidence permits) the subsequent years of adult life. Though the kinds of problems faced in adolescence and the way an individual adjusts to them will be determined largely by what has gone before, the adequacy and full significance of those adjustments can only be evaluated in the light of the type and quality of subsequent adjustments in adult years. A broadened age perspective will, it is hoped, permit a more insightful interpretation of adolescent development than is possible when attention is focused almost exclusively on the teens themselves.'

GEORGE WESTER

The New Man in Soviet Psychology. By RAYMOND A. BAUER. Harvard University Press. 1952. Pp. xxiii + 229. \$4.00. Distributed in Great Britain by Geoffrey Cumberlege. London, at 25s.

This book is number 7 of the *Russian Research Centre Studies*. Its author was formerly Field Director of the Harvard Refugee Interview Project in Europe, and in writing it he has drawn upon his association

with 'scores of ex-Soviet citizens, scholars, psychologists, historians, teachers, people from all walks of life,' as well as upon the printed sources. The story he has to tell may be briefly summarized as follows. In the early years of the Revolution, Russian psychologists worked in an atmosphere, and with a set of ideas, which in the rest of Europe and in the U.S.A. would have been described as 'progressive'. Pavlov's conception of conditioned reflexes was so influential that many even doubted whether psychology was anything distinct from physiology; great stress was laid on the part played by the environment in forming human behaviour; determinism was favoured to such an extent that Freud (who was otherwise not in favour) was approvingly cited against the doctrine of free-will; industrial psychology was pursued with the philanthropic aim of lessening the workers' burdens; more formal methods of education were replaced by 'projects', and there were hopes of 'the withering-away of the school'; psychological tests were widely used; guilt and punishment were regarded as outmoded, un-Marxist conceptions. As a result of the meeting of the Second All-Union Conference of Marxist-Leninist Scientific Institutions in April 1929, and of a philosophical conference held in October 1930, a number of these positions came to be stigmatized as 'mechanistic' and therefore as anti-Marxist. In the years since then, according to Mr Bauer, Pavlov's influence has diminished, and consciousness is regarded as an emergent character not reducible to physiological functioning; the 'two-factor' theory according to which personality is determined by inheritance and environment, was replaced by a 'four factor' theory according to which it is the result of inheritance, environment, training and self-training; although determinism is still adhered to, Freudian theories get no recognition whatever, and there is much talk of man's power of consciously directing his own course of action; the 'philanthropic' element in industrial psychology seems to have greatly diminished, and little work is now published on the psychology of labour; as a result of the decree of 4 July 1936, on 'Pedological Perversions in the System of the People's Commissariat of Education', psychological testing was practically abolished, on the ground that it was an inheritance of bourgeois modes of thought designed to prove 'the special giftedness and the special right to exist of the exploiting classes and "superior races"', and on the other hand, at proving that the working classes and "inferior races", are physically and spiritually doomed'; and a writer on 'judicial psychology' refers to the earlier attempts at abandoning the concepts of guilt and punishment as 'wrecking'.

It will be seen that Mr Bauer has linked his account of Soviet psychological theory with the course of philosophical thought before and after the first Five-Year Plan. This is what Soviet writers would themselves approve of, since they insist that all scientific thought must be in accordance with the principles of Marxism-Leninism. A disadvantage of the method, however, is that specific contributions to the science of psychology may get lost from view in the broad stream of tendencies. It might be useful if Mr Bauer himself, or some other competent person, would select and translate say half a dozen papers on particular problems, and publish them with an introduction, as G. L. Kline has recently done with some Soviet discussions of the philosophy of Spinoza. This might be all the more useful since there is, as Mr Bauer admits, room for some difference of opinion about the direction in which the tendencies really are going. Thus there are some who call attention to the continuing influence of Pavlov, and *Pravda* recently published discussions in which there was mention of his 'second signal' theory. Frolov (*Pavlov and his School*, second impression, 1938, pp. 81-2) says that this was a physiological speculation without decisive experimental support, and Mr Bauer has found very little about it in the stenographic transcript of Pavlov's seminars. Like Mr London in his review of Wortis's *Soviet Psychiatry* (*Psychol. Bull.* XLVIII, 1951, pp. 364-5) Mr Bauer attaches little importance to recent panegyrics on Pavlov, but suggests they may have been stimulated by Stalin's article on linguistics and are an attempt to smuggle semantics—hitherto regarded as 'idealist' in the Soviet Union—in through the physiological back door. No reader of this book can fail to be impressed by certain similarities between the movement of thought in the U.S.S.R. and in Western countries. These are partly due, no doubt, to inadequacies in the theories that were fashionable in the 'twenties, but another cause may well be the similar social problems that have to be faced by countries that attempt central control of their economies. Mr Bauer tells of a Soviet emigré who, when an American sociologist had explained in some detail what sociology is, remarked: 'Oh, that sounds like what we call Marxism-Leninism!'. But clearly Soviet psychology is much more closely bound to practical purposes than is psychology in the West. Thus, when Kornilov defended a view about the relation of mental and physical work on the ground that there was experimental evidence for it and none against it, Zalkind replied: 'This theory (even though it may be "experimentally" established! All the worse: there must have been something rotten in the experiment) is methodologically perverted and leads to harmful practices.' This sort of thing seems pretty brusque to us, even though we may admit that science cannot claim to withdraw from moral judgement, and that the conclusiveness of any single experiment depends upon the framework of theory within which it takes place. One wonders whether any use is made of psychologists in determining and carrying out the policies of the Soviet government.

According to Mr Bauer, by 1936 Soviet psychologists ceased to inquire into political attitudes or to 'probe into the deeper and intimate side of life', but he does not say anything about official inquiries into these matters. His book, besides being most informative, raises important questions about the place of psychology in the thought and practice of our time.

H. B. ACTON

Behaviour Difficulties of Children as Perceived and Judged by Parents, Teachers and Children Themselves. By WILLIAM GRIFFITHS. Minnesota University Press; London: Geoffrey Cumberlege. 1952. Pp. xii + 116. 24s. net.

This monograph reports an investigation which primarily set out '(1) to learn the kinds of behaviour problems which children themselves think they have in comparison with those which their parents and teachers think they have and (2) to study the relationship between common behaviour difficulties and socio-economic status of the children'.

There was a preliminary investigation which took into account earlier work and for the final investigation questionnaires were distributed and completed by 3387 parents. Information was obtained from 900 children of this group of parents, selected on the basis of socio-economic status and chronological age. Teachers completed questionnaires on 760 of the children.

Forty-nine behaviour difficulties were classified in four categories: aggressive behaviour, delinquent related behaviour, withdrawing behaviour and non-compliant behaviour. In the analysis of the items classified in each category sex, age and socio-economic differences are considered, as are children rated as poorly adjusted or difficult to manage. The children's responses are compared with those of their parents and teachers.

Those critical of this kind of research will be left wondering what kind of replies to the questionnaires might have been given by the parents of children who failed to complete them. The proportions were considerable; 35% of parents of elementary, and 58% of junior high school children failed to co-operate in the project.

The method of scoring the ratings was somewhat arbitrary. Parents were asked to mark the behaviour items as occurring 'never', 'sometimes' or 'often', 'sometimes' being given a weight of one and 'often' a weight of two. Inspection of the list of 49 items makes one wonder whether those marked 'never' are not more related to problem behaviour than those marked 'sometimes'.

The book is excellently produced with clear tables of figures. Findings which are statistically significant are indicated at 101 and 105 levels and graphs and histograms make sex, age and socio-economic differences quickly apparent. Indeed the clarity of presentation and lucidly summarized conclusions are a model of their kind. This is an aid both to seeing the results of the research and its conclusions, and to speculation as to the validity of questionnaire methods in general.

N. E. WHILDEN

Saving Children from Delinquency. By D. H. STOTT. London: University of London Press. 1952. Pp. x + 266. 12s. 6d.

This book is a sequel to *Delinquency and Human Nature* in which the author elaborated his somewhat personal ideas about 'delinquent breakdown'. Much that he now says about the treatment and prevention of delinquency is derived from his general theory of human nature and behaviour. He tilts at the psychoanalysts, affirming that the study of personality has long been 'tangled up in Freudian occultism' and that simple truths have been 'clothed in a mysterious priestly language of sexual symbolism'. In his view the principles of modern personality-psychology require no deep analysis; and the best qualification for psychological work in an approved school is, apart from competent membership of the approved school team, 'that sensitivity which enables people to appreciate good literature'. For him the two main human emotional needs are an assurance of personal worth and secure membership of a small belonging group. Starvation of these needs is likely to lead to personality-failure and delinquent breakdown. The aim of treatment is the satisfaction of these needs.

Many readers, particularly those who hold that unconscious factors are significant, may have difficulty in accepting some of the principles which the author enunciates. Moreover, some of his statements are surely mistaken. In London at any rate it is not the case that many adolescent delinquents demand to be sent away. Nor is it correct to say that all the organs of the body, including the brain, develop through the exercise of their powers.

Dr Stott does not believe that mental dullness is even a contributing factor in delinquency and he claims that the whole theory of mental functioning is due for overhaul. These are bold statements in support of which adequate evidence is not adduced.

In a long section on approved schools he attributes existing defects to the lack of any positive guiding principles. The authors of the Munster Report on Approved Schools and Remand Homes may regard this as somewhat uncharitable.

However, the book as a whole contains much to interest social workers. There are sections on the economics of prevention, the community and the school, youth clubs, the work of the Family Service Units, children reared in institutions and the imminently delinquent, containing graphic descriptions and many imaginative suggestions which range from a greater use of foster-homes, perhaps with Trade Union assistance, to parachute jumping.

J.D.W.P.

Measuring Intelligence of Indian Children. By V. V. KAMAT. London: Geoffrey Cumberlege; Oxford University Press. 2nd ed. reprinted 1952. Pp. xvi + 235. 16s. 6d.

This is a second edition of a clear account of a conscientious and intelligent adaptation by Dr Kamat of the Binet-Simon Tests and Stanford Revision for Indian use in the Kanarese and Marathi languages, originally published in 1940 without the two appendices on 'Sex Differences among Indian children in the Binet-Simon Tests' and on 'Heredity and Environment'. Readers of this *Journal* may wish to know whether any striking conclusions on these two latter problems emerge; the answer is no. Sex differences are in favour of boys as against those found by Burt in favour of girls, but the difference is insignificant in populations of 638 boys and 436 girls. Heredity is a factor, environment is a factor, in the intelligence test performances of Indian children.

GEORGE WESTBY

Mental Deficiency. By A. F. TREDGOLD, assisted by R. F. TREDGOLD. London: Baillière, Tindall and Cox. 1952. Pp. xvi + 546 with 48 plates. Eighth ed. 37s. 6d.

A psychologist need not be surprised even now to find a standard medical text-book containing unsatisfactory and out-of-date material in the psychological sections, still less so when that book was first published in 1908 and despite revisions still bears the marks of its years of origin. We gradually hear less of the picturesque genius of Earlswood, J. H. Pullen, and of such anthropometric eccentricities as skull measurements in the case history form (still presumably popular at the time of the fifth edition, 1929). Occasional new names appear in the section on clinical examination along with the old tests of prevision, planning and constructiveness, tests of emotionality, tests of perception comprehension and response, the Word Completion Test of Ebbinghaus, and tests of capacity for mental imagery (under which we have six lines on *The Rorschach Ink-blot Test*, 'probably mainly a test of creative imagination' the author remarks). Scarcely a reader of this *Journal* could avoid wincing continually in reading this section which should be entirely rewritten in the next edition. It is also now impossible adequately to discuss defective and delinquent behaviour on the basis of a Shand-McDougall Instinct Theory.

But the book remains an important monument to the thesis which Dr Tredgold nobly upheld against the mental testers in their 'hubris' of the twenties and thirties—that the final criterion of certification should be social. This was entirely justified and this is a book which has helped to keep this branch of medicine in line with the British empirical tradition.

The purely medical sections are no doubt as satisfactory as ever they were and are, no doubt, the solid basis of the book's continuing popularity, but it is very doubtful whether the table of norms of physical growth and development (Appendix I) is other than useless or even dangerous to-day. It is unchanged from the fifth edition (1929) based on then undated data from W. Preyer, Holt's *Diseases of Infancy* and R. Boyd. The given norms for height and weight for children of school age for instance (which are easiest to establish firmly) are very seriously understated by reference to modern data (such as the L.C.C. Report on *Heights and Weights of School Children 1950*, Sutcliffe and Canham 1950, Krogman 1950 and M.R.C. Report no. 257, 1947).

There is no mention anywhere of Prof. Penrose's *The Biology of Mental Defect* (1949) or any awareness of the important issues it raises, particularly in respect of the genetics of intelligence.

GEORGE WESTBY

Joint Consultation in British Industry. National Institute of Industrial Psychology
London: Staples Press. 1951. Pp. 276. Price 21s.

This is the second major report published by the N.I.I.P. since the war. Like its forerunner, *The Foreman*, it is a factual report of an investigation sponsored by the Human Factors Panel of the Committee on Productivity. The objects of the investigation—which was led by Winifred Raphael—were to find out (a) what goes on in the field of Joint Consultation, and (b) what appear to be concomitants of successful Joint Consultation. There were two sources of information: (i) intensive investigations, lasting up to 5 days, in 180 firms, and (ii) questionnaires completed by over 500 firms. All firms, in both categories employed more than 250 people. Because not all firms in the ideal sample originally selected would co-operate, the conclusions are tentative, but, as the report says, 'the inquiry had a broad enough basis to be of considerable value'. The picture which emerges is something like this: Joint Consultation Committees (often called Works Councils) are usually started on the management's initiative and on the recommendation of an outside authority (e.g. Ministry of Labour); the Committees have about sixteen members, at least half of whom are representatives of the workers elected by secret ballot; office and sales staff tend not to be represented—and apparently do not want to be; the foreman, usually appointed by management, seems to occupy his usual role (see *The Foreman*) of the odd man in; the Chairman of the Committee is normally a member of the management, meetings are held once a month in working time and last one to two hours, and the workers in the shop are mildly, or not, interested in the proceedings; women are poor at it and are even less interested. Successful consultation is associated with: interest of the workers in its proceedings; the Chief Executive of the firm being keen on the idea, and (and this is probably most important) joint consultation being introduced *by* joint consultation—not simply some thing 'put in' by the management.

But no bald summary of conclusions, and indeed no short review, can do justice to this important report. It is full of meat and should be read by all who have views—or who would like to have them—on human relations in British industry. The book is called a report and the draftsman, H. A. Somervell has done a very good job. (Only those who have ever had to turn the material from questionnaires and statistical analyses into coherent and interesting English will realize how difficult his job was.) But I would like someone in the N.I.I.P. to go on from there—to write a book which used this report, *The Foreman* and their other rich sources, simply as illustrative material. Is there a Mayo in the house?

DENIS MCMAHON

Psychological and Psychophysical Studies of Craftsmanship in Dairying. By ROLAND HARPER. British Journal of Psychology Monograph Supplements, no. 28. Cambridge University Press. 1952. Pp. 63. 9s. 6d.

The author is concerned with the judgements of 'firmness' which are important in the manufacture and grading of cheeses and other dairy products; he has investigated both the stimulus features determining such judgements and the psychological ability involved in making them. The technical difficulties which the investigator has had to face are considerable, and while Dr Harper has handled them capably, he is quite aware that the problem still requires further research. It is none the less encouraging to see work of this kind being undertaken. In many industries besides dairying the decision to discontinue a given process, or to award a certain grade to the product, customarily depends, and in some cases perhaps must depend, upon the immediate discriminations of an experienced judge, rather than upon instrumental measurement. By comparison with the manipulatory aspects of craftsmanship, discriminatory skills of this kind have not received the attention they deserve; and it is to be hoped that the present monograph may stimulate other industrial psychologists to make similar studies.

N.E.L.

The Little World of Man. By J. B. BAMBOROUGH. London: Longmans, Green and Co. 1952. Pp. 187. 20s.

This is a short straightforward book about the psychological theories current in English when Shakespeare wrote. It consists of an introduction, four chapters dealing with Elizabethan views on the body, the soul and the relations between them, a conclusion and some useful notes and references. Those who are interested in literature, its relations with psychology and the history of the latter subject should find the book useful. It is lucid, level-headed and, as the author quotes freely from Elizabethan literature, it is enlivened by some very lovely language.

J.W.R.

Psycho-analysis: Evolution and Development. By CLARA THOMPSON, with the collaboration of PATRICK MULLAHY. London: George Allen and Unwin. 1952. Pp. 243. 18s.

The value of this book lies in the fact that we sometimes tend to feel that theories are born fully grown, and so reduce the flexibility and constructiveness of our own thinking. Unfortunately, however, Dr Thompson has not followed psycho-analysis to full maturity, and her book's claim to comprehensiveness is misleading. Her emphasis on the evolution of Freud's ideas is valuable and it is useful to indicate the precise points at which occurred the deviations of Jung, Adler, Reich and others. But the subsequent omissions are glaring. Although, as might be expected in a transatlantic work, there is considerable reference to contemporary writers like Fromm and Horney who live in America, there is not a single reference to object-relations theory or to Melanie Klein. Also, although the social implications of psycho-analysis are discussed in descriptions of the work of Kardiner, Malinowski and H. S. Sullivan, there is no single reference to the application of analysis to group or social therapy. That there should be no mention of these two lines of work, richer than any other modern developments in their implications for both theory and therapy, is strange, but may be excused on the principle that all history—even the history of science—is a personal interpretation based on the writer's individual experience of his own society.

Muscle Relaxation. By GERALD GARMANY. London: The Actinic Press Ltd. 1952. Pp. 65. 5s. 6d.

This book is the first of a series on Physical Medicine written primarily for the physiotherapist. Its relevance to the psychologist lies in the author's viewpoint that 'mental tension is invariably accompanied by perceptible tension in the musculature' and that by learning to relax the somatic musculature one can remove the mental 'tension'. In other words, the author considers proprioceptive impulses from muscle play a large part in emotional experience, without however accepting the James-Lange theory as wholly true.

Whether the more precise connotation that 'tension' has in physiology can be carried over into psychology to encompass anxiety feelings and phobias is another matter. It may well be that psychiatric tension ratings are no more than the perception of generalized muscular tension, in which case relaxing the muscles would indeed lead to a lessening of apparent 'tension'.

The subject-matter is largely confined to instructions for teaching patients to relax. Dr Garmany's patients seem to have literally a harder time of it than those of Jacobson; they lie on a rug on the floor rather than on a soft couch, and breathe to a rigid formula.

Although even the physiotherapist might be well advised to go to Jacobson's *Progressive Relaxation* for fuller information on technique, this is not to detract from a clearly written and sensible treatment of the subject within 60 pages.

D.W.F.

Contrast Psychology. By MURDO MACKENZIE. London: George Allen and Unwin Ltd. 1952. Pp. 304. 21s.

It is best to review this book by quoting from the author's Preface:

'The Freudian believes that the unconscious conflict, underlying an Anxiety State, springs from frustrated "sexuality"; the Adlerian from an obstructed "life style"; the Jungian from the failure of the "collective unconscious" to come to terms with the conscious. Contrast Psychology, on the other hand, asseverates that Persisting Anxiety, within the mind, is invariably the sequence of conflict, beneath the level of awareness, between contrasting components of personality.

The conflict, in individuals, is almost without exception between the rival claims of Immediacy and Deliberation; the innate component is overlaid by its contrast as the sequence of an early parental situation showing contrast and conflict.

Conflict between communities, which gives rise to ideological strife, is composed of the contrasting and competing claims of amplified and simplified groups.

The last chapter contains a psychotherapeutic technique which is founded on the basic tenets of Contrast Psychology. The successful application of this technique involves the interpretation of the Protest and the resolution of the unconscious conflict between the contrasting personality components. Release of the innate components of personality automatically follows; and the once dislocated personality no longer acts in independent isolation, but now functions in co-operation with the mind as a whole.'

R.W.P.

Diseases of the Nervous System. By F. M. R. WALSHE. Seventh Edition. Edinburgh and London: Livingstone. 1952. Pp. xvi + 365. 24s. net.

The Contribution of Medicine to Our Idea of the Mind. The Rede Lecture for 1952. SIR RUSSELL BRAIN. Cambridge University Press. 1952. Pp. 30. 2s. 6d. net.

For psychologists whose work brings them indirectly into touch with diseases of the nervous system having a physical origin, Dr Walshe's book will be familiar as an invaluable work for occasional reference. It is lucid, concise, clearly and consistently arranged. The general psychologist may find it of most interest in indicating the present strength of the rearguard action against psychotherapy. At some points not only is a containing action possible, and Dr Walshe has to give a brief chapter to 'The Psychoneuroses' but its broad tendency is to imply that if the general practitioner's common sense and sympathetic attention (the importance of which is wisely emphasized) are not sufficient for a cure, then there is seldom anything more to be done. It is still possible for the chapter on writer's cramp and other occupational cramps to give no credence whatever to the possibility of the psycho-genesis or the psychotherapy of these conditions; here it would seem that Dr Walshe is underrating the extent of the enemy's advance. But he is not always on the defensive, and he counter-attacks briskly on nocturnal enuresis. Child psychiatrists will admit the effectiveness of some of his thrusts here, although they may still hope for a better treatment than preventing deep sleep by nicely gauged doses of benzedrine sulphate.

While Dr Walshe takes his stand in the last ditch, Sir Russell Brain steps forward with diplomatic urbanity to negotiate a peace, indeed scarcely admitting the existence of a conflict. Freudian psychoanalysis, equally with the work of neuro-physiologists and neuro-psychiatrists is part of the contribution of medicine to our idea of the mind. Freud's 'fundamental principles have been accepted far beyond the limits of his own school, and now form part of the equipment of most eclectic medical psychologists. This Rede lecture is a polished performance, handling the controversial topics of the relation of neurology to psycho-analysis, of psychiatric abnormality to legal responsibility, and of body to mind with tact as probably to give offence to no one.

Notes on the Teaching of Statistics in Schools. By B. C. BROOKES, M.A. London: Heinemann Ltd. 1952. Pp. viii + 80. 5s.

As its title indicates, this little book is designed especially for teachers in schools, particularly for those interested in starting a statistics course but who, themselves, have not had practical experience in handling statistical material. Throughout the book there are suggestions for using information which is familiar and interesting to boys and readily accessible in schools, including results of experiments in various laboratories and data from the geography department, the sports field and the current collecting or the motor-car numbers or railway-engine numbers. The book approaches in simple language such topics as averages, dispersion, frequency distributions and correlation, and includes an adequate introduction to ideas of probability and sampling. Technical terms are introduced carefully and are adequately explained as they are introduced. Although designed as notes which are introductory to an elementary course in schools, to give some preliminary background suggestions to a teacher, they might also serve as an introduction to statistical ideas for a person who wishes to teach himself some elementary statistics but who finds the formal text-book a somewhat formidable beginning. Ideas are introduced here in a straightforward manner, but the book is neither a text-book nor a reference book and it does not stand on its own. For example, regression lines are clearly explained and so is their relation to the product-moment correlation coefficient, but for the actual procedure for organizing data and calculating a correlation coefficient, the reader is referred to a standard text-book such as Yule and Kendall or Weatherburn.

The book is arranged in two sections, the first fifty pages being mainly descriptive and illustrative, serving to introduce the reader to the basic ideas of statistical work, including graphical representation of mean, variance, standard deviation, correlation and sampling. The remaining thirty pages, based on items in G.C.E. syllabuses, serve to help a beginner to find his way through the heavier text-books by supplying simple definitions, explaining necessary technical terminology and giving chapter references in these books. It is essentially a book designed to help a beginner; it does not attempt to go very far, and most of the statistical calculation which a worker in psychology will need is beyond its scope, but anyone concerned with introducing basic statistical ideas should find it a useful guide. F. W. L.